UNIVERSITY
OF MICHIGAN
OCT 17 1953
PERIODICAL
READING ROOM

1831 1953

SED · OMNIA · DISPOSVISTI

YORK · OXFORD · CAMBRIDGE · EDINBURGH · DUBLIN · BRISTOL · LIVERPOOL · NEWCASTLE · BIRMINGHAM · GLASGOW · PLYMOUTH · MANCHESTER · CORK · SOUTHAMPTON · SWANSEA · IPSWICH · BELFAST · HULL · CHELTENHAM · LEEDS

BATH · NOTTINGHAM · DUNDEE · NORWICH · EXETER · BRIGHTON · BRADFORD · SHEFFIELD · SOUTHPORT · MONTREAL · CARDIFF · TORONTO · DOVER · S. AFRICA · WINNIPEG · LEICESTER · PORTSMOUTH · AUSTRALIA · BOURNEMOUTH · LONDON

ABERDEEN · BLACKPOOL

The ADVANCEMENT OF SCIENCE

Vol. X, No. 38

SCIENCE FOR ITS OWN SAKE

Presidential Address by

SIR EDWARD APPLETON, G.B.E., K.C.B., F.R.S.

and

Addresses by

PRESIDENTS OF SECTIONS

Liverpool : 1953

Published by the

BRITISH ASSOCIATION
for the Advancement of Science

Burlington House, London, W.1.

September 1953 Tel.: Regent 2109

Price : Seven Shillings and Sixpence

PITMAN

The M.K.S. System of Units

A Guide for Electrical Engineers

By T. McGreevy, M.Sc.Tech., M.I.E.E., Head of the Electrical Engineering Department, Portsmouth Municipal College. Provides separate approaches to the M.K.S. system for experienced electrical engineers and for students without previous knowledge of electrical units. With numerous reference tables. *Approx.* 25/- *net.*

An Introduction to Chemical Engineering

By T. K. Ross, M.Sc., M.I.Chem.E., A.M.Inst.F., Lecturer in Chemical Engineering at the University of Birmingham. A textbook for the use of all students concerned with the basic methods of chemical engineering.
95 *Illustrations.* *Approx.* 21/- *net.*

Information Theory

And its Engineering Applications

By D. A. Bell, M.A., B.Sc.(Oxon.), M.I.E.E. Covers recent work on the theory of communications systems, and is suitable for telecommunications engineers and for students at an Honours level in physics or electrical engineering. 20/- *net.*

Faster Than Thought

By B. V. Bowden. A general account of computing machines, surveying their history and background, their theory and design, and their uses both in specialized work and in general office work. *Approx.* 45/- *net.*

Antibiotics

By F. A. Robinson, M.Sc.Tech.(Manchester), LL.B.(London), F.R.I.C. A summary of the essential facts about the antibiotics by a research worker with considerable experience in this field. With a bibliography. *Approx.* 15/- *net.*

Experimental Nucleonics

By Ernst Bleuler, Professor of Physics, Purdue University, and George J. Goldsmith, Instructor in Physics, Purdue University. Provides a sound background in the chemical and physical aspects, with useful information on laboratories and equipment and on health protection. 30/- *net.*

SIR ISAAC PITMAN & SONS, LIMITED, *Parker Street, Kingsway, London, W.C.2*

ADLARD & SON LTD.

BARTHOLOMEW PRESS, DORKING, SURREY

Telephone : Dorking 3874

PRINTERS TO SCIENTIFIC SOCIETIES

Experienced in mathematical formulae composition

CIMBRA

(Comércio Império Britânico-Brazil)

IMPORTERS OF

ROCK CRYSTAL QUARTZ

FOR PIEZO ELECTRIC AND OPTICAL PURPOSES

We are at your service PLEASE CONSULT US

CIMBRA LIMITED

123 PALL MALL, LONDON, S.W.1 WHITEHALL 9661 CIMBRALON, PICCY, LONDON

1831 1953

THE ADVANCEMENT OF SCIENCE

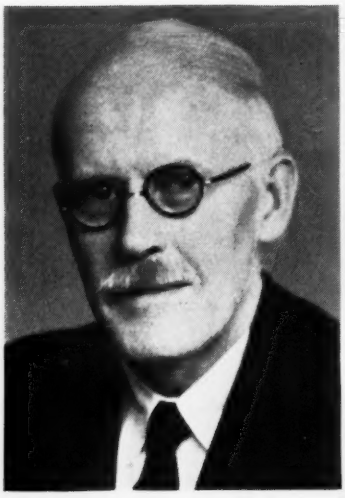
VOL. X, No. 38 SEPTEMBER, 1953

CONTENTS

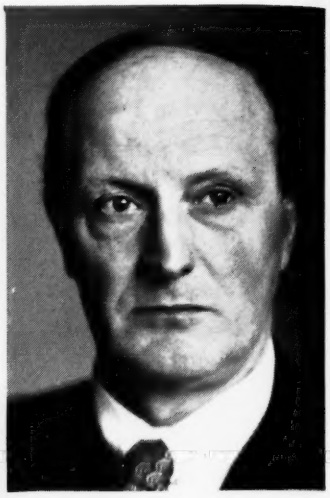
Addresses delivered at the Annual Meeting of the British Association for the Advancement of Science in Liverpool, September 2 to 9, 1953.

	PAGE
PORTRAITS OF THE PRESIDENT OF THE ASSOCIATION AND OF PRESIDENTS OF THE SECTIONS	100
OFFICERS AND COUNCIL	102
SCIENCE FOR ITS OWN SAKE, *by* SIR EDWARD APPLETON, G.B.E., K.C.B., F.R.S.	103
HALF A CENTURY IN GEOPHYSICS, *by* PROF. SIR HAROLD JEFFREYS, F.R.S.	113
A SUGGESTION AND SOME CONSEQUENCES, *by* PROF. G. R. CLEMO, F.R.S.	120
FOSSILS AND THE EVOLUTIONARY PROCESS, *by* PROF. T. NEVILLE GEORGE	132
THE MAINTENANCE AND SPREAD OF SEA-SHORE FAUNAS, *by* PROF. J. E. SMITH	145
THE GEOGRAPHER AS HUMANIST, *by* PROF. R. H. KINVIG	157
OPEN AND REPRESSED MONETARY INFLATION, *by* PROF. F. W. PAISH	169
ENGINEERING IN THE NEXT TWO THOUSAND YEARS, *by* LORD DUDLEY GORDON	181
ANALYSIS AND DESCRIPTION IN SOCIAL ANTHROPOLOGY, *by* PROF. MEYER FORTES	190
QUALITY AND QUANTITY OF PROTEIN FOR MAN AND LIVESTOCK, *by* DR. D. P. CUTHBERTSON	202
PSYCHOLOGICAL PROBLEMS IN THE RECOGNITION OF EXCELLENCE, *by* PROF. D. W. HARDING	213
THE EVOLUTION OF THE OVULE IN THE PTERIDOSPERMS, *by* PROF. JOHN WALTON	223
GREEK OR CHEMISTRY OR BOTH? *by* DR. ROBERT BIRLEY, C.M.G.	231
APPLICATIONS OF 'ANIMAL PHYSIOLOGY' TO AGRICULTURE, *by* DR. J. HAMMOND, C.B.E., F.R.S.	237

Sir Edward Appleton

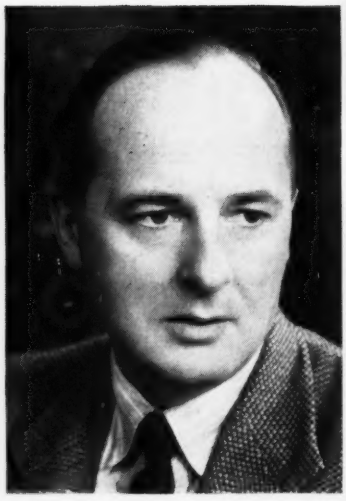


Sir Harold Jeffreys

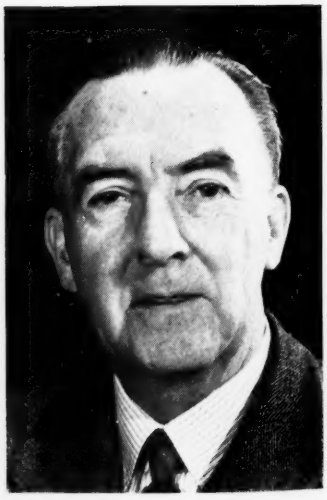


Prof. G. R. Clemo

Prof. T. N. George



Prof. J. E. Smith



Prof. R. H. Kinvig

Prof. F. W. Paish

Lord Dudley Gordon

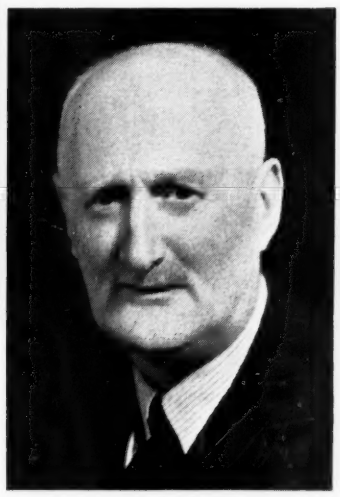
Prof. M. Fortes

Dr. D. P. Cuthbertson

Prof. D. W. Harding

Prof. J. Walton

Robert Birley



Dr. J. Hammond

OFFICERS AND COUNCIL OF THE BRITISH ASSOCIATION, 1952-3

PATRON.

HER MAJESTY THE QUEEN.

PRESIDENT, 1953.

Sir EDWARD APPLETON, G.B.E., K.C.B., F.R.S.

PRESIDENT-ELECT, 1954.

Dr. E. D. ADRIAN, O.M., P.R.S.

GENERAL TREASURER.

M. G. BENNETT.

GENERAL SECRETARIES.

Sir RICHARD SOUTHWELL, F.R.S. | Dr. GEORGE TAYLOR.

ORDINARY MEMBERS OF THE COUNCIL.

Prof. F. BALFOUR-BROWNE.
R. J. BARTLETT.
Prof. L. F. BATES, F.R.S.
M. C. BURKITT.
Wing-Comm. T. R. CAVE-BROWNE-CAVE, C.B.E.
Prof. H. G. CHAMPION, C.I.E.
Prof. WINIFRED CULLIS, C.B.E.
Sir ALFRED EGERTON, F.R.S.
Prof. H. P. GILDING.
Dr. EZER GRIFFITHS, O.B.E., F.R.S.
Dr. D. B. HARDEN.
Prof. K. S. ISLES.
A. GRAY JONES.
Sir HAROLD SPENCER JONES, F.R.S.
Prof. H. D. KAY, C.B.E., F.R.S.
Sir BEN LOCKSPEISER, K.C.B., F.R.S.
J. WICKHAM MURRAY, O.B.E.
Prof. F. W. PAISH.
Prof. W. J. PUGH, O.B.E., F.R.S.
Dr. J. RAMSBOTTOM, O.B.E.
Lord RENNELL OF RODD, K.B.E., C.B.
Prof. L. DUDLEY STAMP, C.B.E.
Dr. W. E. SWINTON.
Dr. H. HAMSHAW THOMAS, M.B.E., F.R.S.
Dr. C. TIERNEY.
Prof. W. WARDLAW, C.B.E.
W. H. WILCOCKSON.
Dr. H. E. WIMPERIS, C.B., C.B.E.
Prof. A. W. WOLTERS.
Dr. N. C. WRIGHT.

EX-OFFICIO MEMBERS OF THE COUNCIL.

Past-Presidents of the Association, the President for the year, the President and Vice-Presidents for the ensuing Annual Meeting, past and present General Treasurers and General Secretaries, and the Local Treasurers and Local Secretaries for the Annual Meetings immediately past and ensuing.

PAST PRESIDENTS OF THE ASSOCIATION.

H.R.H. The DUKE OF WINDSOR, K.G., F.R.S. (1926).
Prof. Sir ARTHUR KEITH, F.R.S. (1927).
Sir HENRY DALE, O.M., G.B.E., F.R.S. (1947).
Sir HENRY TIZARD, G.C.B., F.R.S. (1948).
Sir JOHN RUSSELL, O.B.E., F.R.S. (1949)
Sir HAROLD HARTLEY, K.C.V.O., C.B.E., M.C., F.R.S. (1950).
H.R.H. The DUKE OF EDINBURGH, K.G., K.T., F.R.S. (1951).
Prof. A. V. HILL, C.H., O.B.E., F.R.S. (1952)

PAST GENERAL OFFICERS OF THE ASSOCIATION.

Prof. P. G. H. BOSWELL, O.B.E., F.R.S.
Prof. Sir DAVID BRUNT, Sec. R.S.
Sir HAROLD HARTLEY, K.C.V.O., C.B.E., M.C., F.R.S.
Dr. EDWARD HINDLE, F.R.S.
Sir JOHN LENNARD-JONES, K.B.E., F.R.S.
Prof. Sir JOHN L. MYRES, O.B.E.
Sir FRANK SMITH, G.C.B., G.B.E., F.R.S.
Prof. F. J. M. STRATTON, O.B.E., F.R.S.
Sir ARTHUR TRUEMAN, K.B.E., F.R.S.

HON. AUDITORS.

Sir JOHN SIMONSEN, F.R.S. | Dr. H. J. T. ELLINGHAM.

SECRETARY.

D. N. LOWE, O.B.E.

SCIENCE FOR ITS OWN SAKE

Address by

SIR EDWARD APPLETON, G.B.E., K.C.B., F.R.S.

PRESIDENT OF THE ASSOCIATION

IN expressing my warm appreciation of the signal honour of being elected the Association's President, I would like to add how particularly agreeable the occasion is made for me by the fact that our Meeting this year takes place in Liverpool. In my younger days all my journeys by sea began at Liverpool which thereby came to represent for me a gateway to adventure ; and something of the old magic still remains.

Now, although our Association met four times in Liverpool before the University was founded, in the two succeeding Liverpool Meetings the University has joined with the City in welcoming our members and organising much of our week's activity. Our reception by Town and Gown has been, on this present occasion, notably graceful and gracious, and I would like to tell both the Lord Mayor and the Vice-Chancellor how touched we have been by the cordial expression of their greetings and good wishes.

There is, I think, something very appropriate in our meeting here in Liverpool, a city whose interests and history have been so long and so closely bound up with the advancement of science. This happy conjunction has been remarked on by several of my predecessors and has been a fruitful source of inspiration to them. I have noticed, on looking through their Addresses, how often their thoughts have turned towards the practical achievements of science and how often they have been able to cite their instances from Liverpool itself. It is interesting to note, too, how the city has served, through successive meetings, to point the direction which science was to take. The great developments that were noticed here by the Earl of Burlington at the first Liverpool Meeting in 1837 were an indication of many things to come in other parts of the country before the turn of the century. Indeed, one might say that what was true of Liverpool at one British Association Meeting was true of the rest of Britain at the next, and of most of the world by the one after that.

I confess that I stand somewhat in awe of my distinguished predecessors, who were inspired by this setting to range so widely over the fields of scientific progress in their own day. To succeed to an office which has been held here in Liverpool by such men as Rutherford and Lister and Huxley can be no easy task. It is not only the abilities of these men which now appear so outstanding. What has particularly struck me, on reading their words, is the story of solid achievement each had to relate in speaking of recent scientific progress. 'The heroic age of physical science' Rutherford appropriately calls the period that saw the first investigations of the radio-active elements, the discovery of X-rays and the development of wireless telegraphy. And, again speaking of the period between the two Liverpool Meetings of 1896 and 1923, he remarks 'the epoch has been an age of experiment when the experimenter has been the pioneer in the attack on new problems. At the same time, it has also been an age of bold ideas in theory, as the Quantum Theory or the Theory of Relativity so well illustrate.' Joseph Lister, at an earlier meeting, dwelt in his Address on the same conjunction of experiment and theory, and how their application in the field of medicine had led, in his own time, to the ever-memorable discoveries of anaesthetics and antiseptic surgery. A similar sense of substantial achievement is conveyed in the words of T. H. Huxley. Devoting himself to a theoretical account of the genesis of

bacteria, he nevertheless pauses in it to estimate that the practical applications of the theory, in the French silk-worm industry and wine trade alone, had—to quote his own words—' repaired the money losses caused by the frightful and calamitous war of this autumn '—he was speaking in 1870. In Huxley's remarks, indeed, I seem to detect a certain diffidence about entering the realm of pure theory at all, for, after apologising to his audience for the dreariness of the theoretical approach to his subject, he continues: ' Nevertheless you will have observed that before we had travelled very far upon our road, there appeared, on the right hand and on the left, fields laden with a harvest of golden grain, immediately convertible into those things which the most sordidly practical of men will admit to have value —namely, money and life.'

What my predecessors had to tell was indeed a story of outstanding achievement. And yet what those great men had to say about the benefits of science can surely be equalled, if not surpassed, in our own day. It is true that nowadays we are more sharply aware of the debit, as well as the credit, side of our accounts with science, as my predecessor, Professor A. V. Hill, reminded us last year. But no one could doubt that the material benefits of science are there, and, indeed, are with us in our daily lives to an extent which even the 19th century could scarcely have expected.

Now the subject of my Address, ' Science for its own Sake,' is already known to you. It was chosen to emphasise something that, at times, is in danger of being overlooked, namely, that science has interest as well as utility—that science is illuminating as well as fruitful. Having spent ten years of my own life in seeking to further the applications of science in the practical life of our country, I do not think I can be accused of under-estimating the vital importance of science as an instrument of material utility. I have long held the belief that the cost of scientific research is the price we must pay for our industrial progress. But we should be misleading the public, as well as ourselves, if we based our case for the general support of the pursuit of science on its utilitarian aspects alone. I know that we can claim that many discoveries in pure science, which in their time had no obviously practical import, have later proved to be the foundations of major improvements in our material civilisation. But even that is an argument of profit and loss, and, to my mind, does not bring us entirely to the heart of the matter. I should like to go back, beyond the achievements, to the example of the scientist—be he amateur or professional—who is impelled solely by a passionate desire to explore and understand. That is what I mean by science for its own sake—when knowledge and insight are sufficient reward in themselves. Can the pursuit of a scientific vocation of this kind be a way of living worthily? Can it, in Dr. Johnson's phrase, help to advance us in the dignity of thinking beings? What values for us as individuals does it propound? As well as theoretical knowledge, as well as material benefit, is there some deeper, if more intangible, thing, even wisdom itself, to be found in our vocation?

I certainly make no claim to be able to provide the right, or the only, answers to such questions. At the most I simply hope to indicate to you where I think *some* of the value of a scientific vocation lies. After the extravagant claims that have sometimes been made on behalf of science —claims which have had the disagreeable consequence of putting the scientist on a pedestal—it is well that we should walk humbly. And yet, all the same, in a time of uncertainty of values and lowering of ideals, it is important that we should own to what we believe.

Now I begin by attempting to get the setting right, by pointing out that we scientists do not really inhabit the kind of universe which has sometimes been attributed to us. Science has so often been accused of having reduced the beauty and mystery of the universe to something cold and mechanical. When science enters the door, enchantment, it has been said, flies out of the window. You will remember the lines of the poet Keats :

> ' There was an awful rainbow once
> in heaven :
> We know her woof, her texture ;
> she is given
> In the dull catalogue of common
> things.'

Now it is fairly easy to see how this attitude came about. The world of what is now called classical physics, as it was mapped out for us from the time of Newton onwards, had the advantage of appearing comfortably solid and tangibly final. Anyone, it seemed, who could understand why an apple falls from a tree could also understand the stars in their courses. In this light the universe might well seem to a poet to fail to come up to his expectations. We now know how misleading it was to regard this account of the matter as final. Perhaps the most striking fact about modern science, in its explorations ranging from the heart of the atom to the frontiers of the universe, is that, like poetry, like philosophy, it reveals depths and mysteries beyond—and, this is important, quite different from—the ordinary matter-of-fact world we are used to. Science has given back to the universe, one might say, that quality of inexhaustible richness and unexpectedness and wonder which at one time it seemed to have taken away from it. 'The world will never starve for want of wonders,' says G. K. Chesterton, ' but only for want of wonder.'

I hope to try to illustrate all this in a moment from the fields of cosmical research which have been my own interest for many years. But just now one general point I want to emphasise is that the scientific approach to things is a far more personal and imaginative activity than is sometimes realised. I am ready to admit that deliberate application to discovery can often take us some distance ; also that important progress can result from the operation of a team of workers, as distinct from an individual, though this is mostly the case when the follow-up or consolidation of a basic discovery is in question. But the big jumps ahead are usually the adventures and intuitions of a single mind.

I need hardly remind such an audience as this that scientific activities are twofold. We can make observations and experiments—that is to say, gather facts. And we can also seek to understand how the facts fit together. We express any order we can discern among the welter of facts in the form of a hypothesis or a theory. A theory, by the way, is only a hypothesis that has become, so to speak, respectable. But even then there is nothing final about it. As J. J. Thomson once said, a theory is a policy rather than a creed.

Now, even in this question of making observations, the scientific process is one which requires the fullest and subtlest employment of all our faculties. It demands, for example, that we should not only see things, but should notice them ; and not only notice, but perceive them. Many a vital discovery has been nothing else than recognising the unexpected. To encounter nature in this necessary state of awareness is inevitably to find all its forms and movements, from the infinitely small to the infinitely large, full of inexhaustible significance and relevance. But even in experimental work it is the primacy of an imaginative idea or intuition that often starts it all off. In simple words, I might say that the important thing in experimenting is to ask nature the right question and in its most direct form. Only then is the answer clear and unmistakable. But so often one has failed to ask the right question and the terms of it have to be recast. In this complex process it is as if knowledge were playing a game of chess with the mind, and one has to be constantly on the alert with fresh tactics or even a changed strategy.

Many of our questions turn out to be wrong because they are unanswerable, but it is only by asking them at all that we eventually find we have asked the right one. And one knows how oddly, how unreasonably I might almost say, the right question has often flashed into men's minds. It was recorded by the German physicist Helmholtz that his best ideas only came to him when he was walking up a slowly ascending street—and significantly enough he was one of the founders of the principle of the conservation of energy ! What I do know, from my own experience, is the fruitlessness of pondering over a scientific problem too long. The mind gets polarised and thought becomes captive to a groove. How often the best way of solving a scientific difficulty is to leave it alone ! Also, speaking as a professional scientist who has only recently turned amateur, I would like to acknowledge the immeasurable debt which science owes to members of the latter category. My own subject of radio-physics has, on

many occasions, been advanced by the observations of the gifted and enthusiastic amateur who was able to recognise the unexpected, even if his professional skill was insufficient to enable him to reveal its full meaning and implication.

Asking nature the right question in the right way—or recognising a theoretical pattern in a tangled skein of experimental data—often has the effect of introducing an element of beauty and elegance into the scientist's work. Do we not, on occasion, refer to a ' beautiful theory ' and an ' elegant experiment ' ? It is perhaps a little difficult to say what precisely we mean by this. Not, I think, that the theory or the experiment is necessarily conclusive or irrefutable, or even particularly fertile in its consequences—that would be virtue of another kind. The quality I have in mind is that of inevitability—and yet, paradoxically enough, an inevitability which can cause surprise ! A great experiment seems to us, somehow, something which could not have been done differently. Or, if it had, something essential would have been lost. We are surprised that someone thought of doing it that way but we can see now that that way is really the only way to do it. Taking away something, or adding something, only detracts from it. In this respect a beautiful experiment can surely be classed with a great work of art.

Now I have spoken in general terms of the scientist's approach to nature and of the kinds of mental quality and awareness that science requires in its followers. And I have tried to suggest that the exercise of these skills has a value in itself which is ample justification of a scientific vocation. To go further might be claiming too much. And yet, I wonder. If we think of the great figures of science and, to be fair, restrict ourselves only to those we have known intimately, can we not go further and say that the scientific vocation, by its very nature, calls for personal qualities that deserve to be recognised and honoured ? I might point, for instance, to that tolerance and open-mindedness to new ideas which shines even from the printed page of Rutherford's Address to this Association when he was President here thirty years ago—that freedom from prejudice, muddle, hypocrisy and darkening of counsel which characterised the man many of us were so privileged to know.

And yet, in less serious vein—a vein to which Rutherford was as much addicted as anyone else—ought we not, as scientists, to try to see ourselves as others see us ? Joseph Addison once declared that there was this at least to be said for natural philosophy, that it occupied the attentions of men, who, if they had pursued public affairs and politics with equal zeal and vigour, would have set the whole country aflame. While Dr. Hartley in his *Observations on Man* declares that ' Nothing can easily exceed the vain-glory, self-conceit, arrogance, emulation, and envy that are to be found in eminent Professors of the Sciences, Mathematics, Natural Philosophy, and even Divinity itself. Temperance in these studies is, therefore, evidently required, both in order to check the rise of such ill passions, and to give room for the cultivation of other essential parts of our natures.' And yet Adam Smith, in his *Theory of Moral Sentiments*, finds far more to say in our favour. ' Mathematicians and Natural Philosophers,' he says, ' from their independency upon the public opinion, have little temptation to form themselves into factions and cabals, either for the support of their own reputation, or for the depression of that of their rivals. They are almost always men of the most amiable simplicity of manners, who live in good harmony with one another, are the friends of one another's reputation, enter into no intrigue in order to secure the public applause, but are pleased when their works are approved of, without being either much vexed or very angry when they are neglected. It is not always the same case with poets, or with those who value themselves upon what is called fine writing.' I only hope we scientists can see ourselves in that mirror.

For specific examples of what I have called the pursuit of science for its own sake there is, of course, no shortage of material on which to draw. I have, however, decided to tell you the story of only one field of development to-night—a long short story if you like—instead of a number of short stories in brief outline. My story has been selected because it bears on what men think about the world

rather than what they do about it, for it concerns the nature of certain objects in outer space whose nature has only been revealed in recent months. If I required a more homely title for my story it would be ' Finding things out about places we can't visit '.

From time immemorial men have examined the sky with their eyes, and found it to be populated with luminous bodies, the stars shining with their own, and the planets with borrowed, light. As time went on, telescopes were used to assist the naked eye, and in this way it was possible to see more feeble and more distant stars. Generally we may say that the bigger the telescope the more powerful it is in helping us to plumb further into the depths of space. The 200-inch telescope at Mount Palomar can detect stars so far away that it takes the light from them 1,000 million years of travel to reach us. In such cases the human eye is supplemented by the photographic plate which, through prolonged exposure, permits the photography of faint objects which can never be detected by the eye alone. In addition to the telescope the astronomer has also looked at stars with a spectroscope, by which the light from the stars is analysed into its constituent colours. In this way it has been possible to identify the kinds of atoms which exist in stars ; since we know, from experiments carried out on the earth, how to recognise particular atoms by the particular colours—or wave-lengths—of the light they give out. We can call all observations of this kind optical astronomy since, in making them, we examine the optical light which comes from the stars.

Now it is of great interest to us here in Liverpool to-night to recall that it was Sir Oliver Lodge, one of the first professors in Liverpool University, who first thought of looking at the heavens with a ' radio eye ' instead of with an ' optical eye '. The year was 1900, in the earliest days of radio, and Sir Oliver tried to discover whether he could detect radio waves from our own particular star, the sun itself. The experiment failed because of the insensibility of the wireless receiver used—it was the day of the coherer. Yet it would be no misnomer to call Sir Oliver the first radio-astronomer, for the experiment was surely conceived on right lines and, with modern valve equipment, would have commanded success.

However, it fell to an American radio-engineer, the late J. G. Jansky, of the Bell Telephone Laboratories, to discover that radio waves, as well as light waves, could be received from the heavens. One interesting feature of this discovery was that Jansky found all this out when he was looking for something else. But with impressive scientific awareness he was ready to recognise the unexpected. Jansky was primarily studying the direction of arrival of atmospherics, but he noticed a persisting hiss in his ear-phones when his directional aerial was aligned to receive from a particular direction, which he later showed was the direction of the stars in the Milky Way. The year was 1931.

Jansky, appropriately enough, was the first person to speculate on the origin of this radio noise from outer space. He pointed out that the most obvious explanation was that the radio waves came from the stars themselves and that we get the radio noise in strength from the Milky Way because a great population of stars is concentrated there. But when he came to test his hypothesis by looking for radio waves from our nearest star, the sun, he obtained a discouraging result. For, even with the equipment available in his day, he found, like Sir Oliver Lodge, that no radio waves were detectable.

Now it is one of the remarkable features of the history of this subject that Jansky's researches incited only a few sporadic observations in the way of repetition. During the Second World War, however, radar operators, using their sensitive equipment operating on wave-lengths of 5 to 10 metres, so to speak re-discovered the phenomenon. But that was a period when one had to distinguish sharply between the scientific things that were only interesting as distinct from the things that were really useful. Quite a number of other matters of purely scientific interest arose in the same way, but all one could do was to note them and shelve them till the war was over.

I can well remember how these various topics cropped up, during the War, in the discussions of a panel of young scientific

workers of which I had the good fortune to serve as Chairman. This panel was really a small sub-committee, though it had a long name[1] and a distinguished parentage. It used to claim that, unlike most other war-time committees, its membership was confined to scientific workers and did not include officials. That I might have been classed in the latter category was generously overlooked. The task of the Ultra Short Wave Panel was to examine and interpret all the manifold vagaries of radar transmissions which were reported to it from operational experience and from *ad hoc* experiments. The Panel had to concern itself with many things including, for example, the profound influence of the weather on radio wave travel in the lower atmosphere. The foundation of a new subject, that of radio-meteorology, was one result of its labours. That was a matter of practical moment. But the odd bits of information on radio-astronomy, though choice delicacies for a scientific appetite, had to be renounced. I have in mind here, in addition to the detection of radio noise from the Milky Way already mentioned, such subjects as the radar detection of meteors or 'shooting stars' and the detection—for the first time—of radio waves of violent intensity coming out of sunspot regions on the face of the sun.

Soon after the War, however, these matters became the objects of further enquiry and it is of much interest to note that radar equipment, developed in the first instance for the detection of aircraft and ships, proved extremely useful for this purpose, requiring only minor adaptations. Dr. J. S. Hey and his colleagues, S. J. Parsons and J. W. Phillips, for instance, made the first really detailed investigation of the amount of radio noise coming from different parts of the sky. For this purpose they converted a war-time radar receiver, which had been used in conjunction with anti-aircraft batteries, into a directional radio-telescope. Their work at once confirmed Jansky's original findings and showed that there was a close agreement between the intensity of the radio emission and the distribution of visible stars in the Milky Way.

Now I must digress from my main theme for a moment to remind you of a little of what is known about the distribution of stars in the universe as a whole. Our own solar system is really part of the Milky Way which is, itself, a colony or island of stars in space. This island colony has a structure like a magnifying glass, so that it is circular in shape but thicker at the centre than at the edges. You would not be far wrong if you thought of it as a Yorkshire tea-cake, the currants of which represent the stars. We inhabitants of one of the planets in the solar system do not occupy, however, a particularly privileged position in it, for we are situated nearer to the edge of the island colony than to its centre.

Space is, however, populated with far more stars than are to be found in our own Milky Way. But they are not distributed uniformly. They are grouped in island colonies exactly like our own. The astronomer calls these star colonies extra-galactic nebulae, and, with our big modern telescopes, it is possible to detect between 100 and 1,000 million of them. The average distance between any one of these star colonies or nebulae and its nearest neighbour is about ten to a hundred times the size of either.

But I must resume my detective story about the radio noise from the Milky Way, for I think you will agree that it sounds much like a detective story as one clue after another is followed up. It was natural to assume at first that the radio noise coming from the sky represented the integrated radio effect of the stars in our own galaxy, since such stars are nearest to us. Our war-time experience concerning radio noise from the sun helped with the necessary calculations, for one could assume, as a first approximation, that all the stars would act like the sun. However, it turned out that there did not appear to be enough stars in the Milky Way to account for the high intensity of the radio noise. So then, as an alternative hypothesis, it was thought that possibly the noise came from the flying atoms and electrons which we know must populate the space between the stars. This was the inter-stellar matter theory. Unfortunately,

[1] The Ultra Short Wave Panel of the R.D.F. Applications Committee of the Advisory Council for Scientific Research and Technical Development of the Ministry of Supply.

here again, there were found to be difficulties. To account for the high intensity of galactic radio noise, when observed on the longer radio wave-lengths, required the ionised gas in inter-stellar space to be at a temperature of 100,000 degrees absolute, a value much too high to be reasonable. Also, we already knew that the inter-stellar material in question is concentrated in a narrow band near the galactic equator quite unlike the wide dispersal of both the stars in the Milky Way and the radio noise. So another theory had to be abandoned.

We had therefore arrived at this position, that the cosmic radio noise could not be accounted for as coming from the visible stars in the Milky Way or from the tenuous material existing in the spaces between the stars. However, this unpromising situation was soon relieved by an experimental discovery which, in its turn, led to others. Dr. J. S. Hey, observing the intensity of radiation from different parts of the sky, noticed that the strength of the radio noise from one particular direction — from a region in the constellation of Cygnus — occasionally showed rapid variations in a period of about a minute. At first it was thought that these fluctuations indicated variations in the emission from the source itself, but we now know that the variations are brought about by irregularities in the ionosphere, through which, of course, the radio waves must pass before they reach us on the surface of the earth. We can, in fact, look upon these variations as a kind of ' twinkling ' introduced by irregularities in the atmosphere. But the most startling conclusion which could be drawn from these observations was that the source of this variable radio noise must be a very small one. In the case of visual light we all know that we can have a ' twinkling ' star—because the star is a small source—but not a ' twinkling ' moon.

Attempts were therefore immediately made to find out, by even more refined radio experiments, how big—or how small—in size this radio source in the direction of Cygnus actually was. There is no time to describe to you to-night the apparatus which was used, in Australia and in Cambridge, to test this matter but I can assure you that both experiments qualify for my adjectives ' beautiful ' and ' elegant '. Both sets of investigators announced the same result, that the source in Cygnus was too small to have its size assessed with the equipment used—that is, that it occupied less of a tenth of a degree in the sky. However, in the course of the same experiments, the position of this powerful radio source was fixed pretty accurately. And maps of known stars were eagerly consulted to see if the Cygnus radio star, as we may now call it, coincided with any special visual emitter. The result of this examination was most significant for it was found quite impossible to identify the radio source with any particular star. Within the region which contained the radio star there were many faint visual stars to be found, but none of them seemed to exhibit any special peculiarities likely to associate it with the very powerful intensity of the radio emission.

Then, other parts of the sky were examined with the same apparatus and a further number of ' point ' sources of radio waves were identified. One was found in the constellation of Cassiopeia, which was even more powerful than that in Cygnus. It also could be located with good accuracy. But, here again, there was no remarkable visible object apparent on the star map to connect it with.

It is important to pause here a moment to consider the effect of these observations on our outlook at the time. It was natural to ask a whole series of questions. Could it be that a radio star is always a dark star—and so a new type of object in the universe ? If so, could it be that there was a duplicate universe—only to be seen with a radio-telescope as distinct from a visual telescope ? And, as regards the overall phenomenon itself, could it be that the total emission of radio waves from the galaxy might be really the integrated effect of these dark stars, just as the diffuse band of light of the Milky Way arises from the unresolved radiation from distant optical stars ? You will see that the tendency of the time was still to think of the radio emitters as being neighbouring bodies and inhabitants of our own galaxy rather than of other, more distant, island colonies or nebulae. However, a little reflection will show that we must not rule

out the possibility of radio nebulae, as distinct from individual radio stars, even if we suppose that the greater part of our own radio noise comes from our own galaxy, the Milky Way. Any observer well outside the Milky Way should be able to detect the radio emission just as we, who are situated inside it, can do. Such an argument received strong experimental support when a group of radio-astronomers in Manchester noted a relatively faint, but quite detectable emission, from the Andromeda nebula, which is a neighbouring stellar island colony of our own. Further work, at Manchester and Cambridge, has revealed appreciable radio emission from other nebulae not far from our own galaxy. It is clear that, in such cases, we are probably detecting their internally generated radio noise from the outside.

But the problem of the much more powerful sources remained. Where were they, why were they so powerful, and were they, basically, radio stars or radio nebulae? Again it was a case for more refined experiments which would enable their positions to be found with greater precision and give some notion of their sizes. I might mention here, in passing, that workers in Sydney, Australia, under the lead of J. G. Bolton, had already tentatively identified one radio star with the Crab nebula, a diffuse, expanding, cloud of gas which represents the remains of a stellar explosion, visible news of which reached the earth, according to Chinese records, on July 4, 1054 A.D. So this was another radio source identifiable with a visible object, but it was of abnormal type, the ancient relic of a supernova eruption.

The real attack on the identification of the two major radio sources, those in Cassiopeia and Cygnus, depended on the more accurate identification of their positions. In 1951 some new determinations were made by F. G. Smith at Cambridge and the results were communicated to the optical astronomers with an invitation to search afresh the parts of the sky in question. In the spring of 1952, a new and intensive optical search was therefore undertaken by Baade and Minkowski, at Mount Palomar, using the 200-inch Hale telescope and the 48-inch Schmidt telescope. As a result of this meticulous search, two entirely unknown objects were discovered in the universe. The Cassiopeia radio source was found to be associated with a diffuse cloud of luminous gas, situated within our own galaxy, and possessed of unique characteristics. The tenuous matter of which it is composed is concentrated in a number of fine filaments which are in the most violent motion. From a study of the colour of the light emitted by different parts of the same filament it is concluded that the velocities of such movement are of the order of several thousand kilometres per second. The origin of this gaseous cloud is unknown for it seems impossible to regard it as yet another supernova explosion.

The source in Cygnus, which you will remember was the one which first gave the clue to the possibility of radio stars, was found to be an entirely different type of object. Here the source of the radio waves was identified with another exceptional object which is considered to be two island colonies—two extra-galactic nebulae—in collision. Moreover, the distance away of this compound group of stars is estimated as being such that it requires 100 million years for the light and the radio waves generated in it to travel to us here on the earth. It is rather a humbling thought that it is only during the last sixty years of that travel that human beings have managed to learn how to produce radio waves and receive them. Now when a collision of two nebulae takes place it is considered that the stars of one island colony will pass freely between those of the other. On the other hand, the more extensive inter-stellar materials of the two island colonies will meet in collision, which will result in high excitation of the gaseous atoms of which this material is composed. One must suppose that, in both the radio source in Cassiopeia and the colliding nebulae in Cygnus, the high gaseous velocities give rise to this intense radio emission, although the detailed mechanism by which it all comes about is not understood. These identifications were finally confirmed by observations in Manchester, Sydney and Cambridge using still more refined methods of finding the sizes of these radio sources. The results of all three radio-observatories

were gratifyingly consistent and were published simultaneously last December. In all cases it was found that the radio sources examined were definitely much bigger than simple stars and therefore corresponded to the sizes of the objects observed optically.

The accurate location of these radio sources in the universe—we must now, I think, drop the term ' radio stars '—has therefore led to new discoveries of great astronomical interest. Two unknown objects of unique character have been identified in the heavens, as a result of clues from the radio side ; and it is to be expected that future accurate measurements of the positions of these cosmic radio emitters will lead, in turn, to the discovery of other visual objects of uncommon types. I should explain that the discovery of these rare objects by direct visual search with large optical telescopes would require quite prohibitive effort. The radio-telescope has therefore shown itself to be an important adjunct to the world's greatest optical telescope. But, in addition, there is a further and far-reaching possibility. It is the astronomer's ideal to reach, with optical ranging, the hypothetical limit of the expanding universe, the distance where the extra-galactic nebulae are receding from us with the velocity of light. So far he has reached very approximately half-way. But the fact that, already, the second most intense radio source can be detected without difficulty at a distance equal to one-tenth of the maximum distance plumbed by the 200-inch telescope suggests that it may, in time, be possible to detect sources at greater distances by radio than by optical means.

But, in any case, the more detailed radio mapping of the radio sources in the heavens must go on ; for, as you will have gathered, we are still without an explicit solution of the original problem which started it all off, namely the rough overall correlation of the distribution of radio noise with the general structure of the Milky Way. Much progress in these matters can, I am sure, be expected from the operation of two large British radio-telescopes. Professor A. C. B. Lovell's group is constructing a large steerable paraboloid of 250 feet diameter at Jodrell Bank, a station of the University of Manchester, which will be the largest single radio-telescope in the world and available for a great variety of investigations ; while the large interferometric radio-telescope, recently completed at Cambridge, for work by M. Ryle and his associates, is already yielding entirely new results in the detailed mapping of radio sources.

The radio-astronomical story is therefore far from being fully told, but even already one can record achievements in these three centres of Cambridge, Sydney and Manchester—partly in competition and partly in collaboration—worthy to be ranked with the greatest feats in the art of experiment.

I have only a few words to add by way of postscript. I have tried to show how science, pursued for its own sake, can enlarge men's horizons and invest the world with deeper significance. As an exercise we can claim it to be one of the most complex and far-ranging of our mental experiences. But we must not forget that there are other values and other experiences. At the opposite pole from our scientific endeavour there are the ways of thought which do not change, whose concern is with what is *not* new, with the things that will not be superseded ; and to-day we stand in need of these enduring and sustaining values of the spirit more than ever. We well know that, in the field of science, our work will in due course be probably out-dated and certainly surpassed. At any one moment we may have only a precarious hold on a temporary truth and our consciousness of this ever urges us to seek fresh truths and new understandings. I fear that, in doing so, we may lose sight of other aspects of life which have their values too. For, you know, there is a virtue in contentment, in being satisfied with what we already have, which we shall not learn from science. Our vocation, in other words, cannot be the whole of life for it cannot satisfy all our needs. Nevertheless, I hope I have represented it fairly as no unimportant or unworthy part of it. For we scientists are specially fortunate in this, that our vocation can never be simply an occupation ; it is, by its very nature, more than that—a dedication to an end. It often seems to

me that what we lack in the world to-day is not so much the impulse to dedication as the opportunity for it. This, at least, the scientist need never lack ; the opportunity is open to him everywhere ' to strive, to seek, to find and not to yield '. To those words of Tennyson's Ulysses I would add the words which Dante, long before, put into the mouth of the same Ulysses, when he encourages his crew to venture with him, beyond the furthest point of the known world :

> ' O, brothers,' he said, ' who through a hundred thousand dangers have reached the West, deny not, to this brief vigil of your senses that remains, experience of the unpeopled world behind the sun. Consider your origin : you were not formed to live like brutes, but to follow virtue and knowledge.'

To Ulysses and to Dante ' experience of the unpeopled world behind the sun ' meant, of course, the adventure of voyaging out into the Atlantic, where the sun set and the world came to an end. To-night, as I have tried to show, the scientist has commissioned the words to take on a different—and, indeed, a quite literal—meaning.

But we must still ask ourselves what it is that urges men to do these things and our answer must surely be that it is the challenge of it all. Why should anyone want to climb Mount Everest ? Simply, I suggest, because it is there—as a challenge of the unknown and the unaccomplished—a challenge to spirit and body, now so gloriously met by Hillary and Tensing. In its different setting, the pursuit of science also presents to the human mind an enduring challenge on an endless frontier, quite apart from the material enrichment of mankind to which it may incidentally give rise. ' The work may be hard, the discipline severe,' as Lord Rayleigh said on an occasion similar to this, nearly seventy years ago, ' but the interest never fails and great is the privilege of achievement '.

HALF A CENTURY IN GEOPHYSICS

Address by

PROF. SIR HAROLD JEFFREYS, F.R.S.

PRESIDENT OF SECTION A

AT the middle of the century it seems interesting to look back to the beginning and see what changes have taken place. I was hardly scientifically conscious in 1900, but fortunately there are plenty of records of what people were thinking then; and I think that later developments have been interesting not only with regard to the history of geophysics, but to much wider questions of scientific method.

When the century began the dispute concerning the age of the Earth was still raging. Helmholtz had shown that known chemical sources of energy could not have supplied the Sun's radiative energy for more than a few thousand years, but that gravitational energy released by contraction would have been enough to maintain the Earth's surface at its actual temperature for about 20 million years. This appeared to be the greatest duration possible since the Earth acquired an ocean. The result was confirmed by Kelvin's argument from the cooling of the Earth. This showed that an originally molten body, after it had solidified, would take about the same time before the temperature gradient at the surface could sink to the observed value. On the other hand, geologists, from various data for total amounts of denudation, and assuming a constant rate of denudation, got about 300 million years. Both were equally convinced by their own data and unable to see any point in each other's. The controversy actually started about 1850, and the most often quoted remarks on it were made by Huxley in a presidential address to the Geological Society in 1869. They are worth repeating both for what they say and for what they do not say.

'I do not presume to throw the slightest doubt upon the accuracy of any of the calculations made by such distinguished mathematicians as those who have made the suggestions I have cited. On the contrary, it is necessary to my argument to assume that they are all correct. But I desire to point out that this seems to be one of the many cases in which the admitted accuracy of mathematical processes is allowed to throw a wholly inadmissible appearance of authority over the results obtained by them. Mathematics may be compared to a mill of exquisite workmanship, which grinds you stuff of any degree of fineness; but nevertheless, what you get out depends on what you put in; and as the grandest mill in the world will not extract wheat flour from peascods, so pages of formulae will not get a definite result out of loose data.'

I shall return later to the treatment of inaccurate data. But Huxley's application suggests that the inaccuracy was in the physical data and not in the geological ones. This was not so; Helmholtz's data were the Sun's mass, size, and rate of radiation, and modern estimates of these are not seriously different from those available in 1850. Kelvin's data were rough, but were good enough to give an answer to a factor of 2, and he admitted the possibility of a factor of 5. The denudational data were much vaguer. The total amount of sediments and the present rate of denudation might each easily have been wrong by a factor of 3, and the identification of the present rate with the average through geological time was utterly uncertain. I know of no reply to Huxley by a contemporary physicist, but one on these lines would have been possible.

Huxley's reference to the mathematical mill, again, is disastrously incomplete.

The mathematical method works both ways. An engineer or a dynamical astronomer may be sufficiently sure of his data and principles to be willing to predict the performance of a bigger power plant than has ever been made before, or to predict the motion of a planet a century ahead. But in any subject the stage of prediction is preceded by that of explanation. Hypotheses must be formulated, and their consequences must be developed. If the consequences agree with facts, we may say that the hypotheses provide an explanation of those facts. (Not *the* explanation—several hypotheses may explain them equally well.) Working out the consequences of a set of hypotheses is a task of applied mathematics—a set of hypotheses whose consequences are different from the facts we want to explain is no explanation at all. I quote Lord Rayleigh : [1]

> 'As more impartially situated than some, I may perhaps venture to say that in my opinion many who work entirely upon the experimental side of science underrate their obligations to the theorist and the mathematician. Without the critical and coordinating labours of the latter we should probably be floundering in a bog of imperfectly formulated and often contradictory opinions.'

The trouble is due in part to the emotional expression 'exact science.' Much of pure mathematics is exact ; much consists of giving bounds to the errors of approximations. But no measurement is exact, and there is no exact science based on measurement. Applied mathematics is perfectly capable of allowing for uncertainties in the quantitative data. Huxley's implication is that the hypotheses used as a starting-point are treated as certain and exact. Actually the first task of applied mathematics is to test hypotheses by comparison of their consequences with facts. The proportion sum done in the denudation estimate was also mathematics. Mathematics is just as much a tool of scientific inquiry as the microscope. It never by itself proves observable results : it tests their interrelations.

[1] Presidential address to the Royal Society, 1906.

The discovery of radioactivity about 1900 changed the situation completely. It was found that with radioactive heating the temperature gradient would take longer to sink to a given value, so that Kelvin's estimate of the age might well be too low. But radioactivity also led to constructive results of its own. Uranium and thorium were shown to generate lead at rates that could be measured in the laboratory. Analyses of old rocks hence gave their ages, and several were found by 1910 that had ages well over 1,000 million years. The absolute ages so found fell, on the whole, into the same order as the relative ages found by geological methods, and the agreement convinced many physicists that the ages were genuine even before it was known what was wrong with the Helmholtz estimate. Conversion of hydrogen to helium was suggested by Perrin and Eddington in 1920 as an additional source of solar energy, but a theoretical explanation of such conversion was not available until the work of Bethe and Gamov in 1939. It is noteworthy that Jeans, in his *Problems of Cosmogony* (1919) still accepted the contraction theory and its consequences.

Actual rocks are known now with ages near 2,000 million years. Holmes has given an argument based on ratios of abundances of lead isotopes that suggests a total age of the Earth of about 3,300 million years, but the data include very few analyses of really old rocks, and for intermediate ages there are some exceptional specimens ; until there are more data I should hesitate myself to say that the age is much over 2,000 million years or known within 500 million years.

The denudational estimate was revised by Holmes in 1912 ; he pointed out that denudation at present is very likely to be five times as fast as the average of the past, and the results are consistent with the estimates from radioactivity.

It may seem unfair to criticise an address nearly a century old, but it is necessary because its spirit still animates much of present controversy. Applied mathematicians are attacked for going beyond their data ; if an engineer accepted that he would never design a new machine. A more subtle form of the criticism is that science is concerned only with inferences

that can be verified by observation. One answer is that we cannot be sure what new types of observation may become possible. A more fundamental one is that once we get beyond the immediate data of sensation, everything is inference ; in ordinary speech we say that we observe a physical object, but it takes a great deal of analysis of the processes of inference before we can even say what we mean by observing a physical object ; and when we have got so far it seems that the most remote inferences involve nothing new in principle. The only way of testing a scientific hypothesis is to develop its consequences and compare with observation, and the further we push it the better chance any incompleteness will have to show itself. That is the only way of testing the generality of a scientific law. If two independent hypotheses lead to the same answer, that may be regarded as confirming both ; the alternative would be that, for completely independent reasons, they were wrong by the same amount. That can happen, but it is a freak. The remarkable thing about the Helmholtz-Kelvin estimates is that it is just what did happen—they were wrong through undiscovered phenomena of nuclear physics, not through uncertainty in the quantitative data. But much discussion based on the later developments amounts to turning the exception into a rule that would reject most of science. Before the discovery of the new sources of energy it was occasionally suggested that some unspecified unknown source might resolve the difficulty; but until the properties of such a source were formulated it remained an unfruitful speculation. The fruitful speculations are those that are sufficiently clearly stated to have their consequences worked out. This became true of radioactivity about 1906 ; of the synthesis of helium in 1939. I am not condemning either the physicists or the geologists of the 1850's for the constructive statements that they made. On the contrary, I say that they both had grounds for those statements and that it was their duty to make them. What I do condemn is their over-confidence, which led to a separation of two interrelated sciences whose co-operation was essential to both.

The mass and radius of the Earth and, therefore, its mean density have been fairly well known since about the middle of the eighteenth century. The greatest part of the uncertainty of the mass comes from the laboratory determination of the constant of gravitation, which, however, is now known to about 1 part in 1,000. The moments of inertia are also known. If A, B, C are the moments of inertia about two principal axes in the equator and the polar axis, the rate of the precession of the equinoxes determines the ratio

$$\frac{C - \frac{1}{2}(A + B)}{C}$$

which is consequently well determined. The external gravitational field contains a term depending on $C - \frac{1}{2}(A + B)$, which is theoretically connected with the Earth's ellipticity, the variation of gravity with latitude, and with three observable perturbations of the Moon. Consequently the ratio C/Ma^2 is known, where M is the mass and a the radius. For a homogeneous nearly spherical body it would be $0{\cdot}4$; the actual value is $0{\cdot}334 \pm 0{\cdot}001$, so that the density must increase considerably towards the centre. Alternatively, if we assume hydrostatic conditions in the interior, we can choose density distributions such that the actual rotation leads to the right value of $\frac{C - \frac{1}{2}(A + B)}{C}$. Such data on the density distribution were used, in various ways, by several writers before 1900 ; the most interesting is the hypothesis of Wiechert, that the Earth consists of a uniform core surrounded by a uniform shell. He found that the data would be consistent with a wide range of constitutions, but he preferred the values : shell density $3{\cdot}2$, core density $8{\cdot}2$, core radius $0{\cdot}78a$. The densities appeared plausible because they are near the densities of the stony and nickel-iron meteorites, and the adopted shell density is near those of the denser silicate rocks that we know.

The possibility of much more detailed knowledge on the structure of the Earth began in 1901. As long ago as 1829 Poisson had predicted that an elastic solid could transmit both longitudinal and transverse waves with different velocities, and toward the end of the century numerous observatories were engaged in

trying to detect them. The longitudinal ones have the greater velocity. The actual discovery, however, was not made until 1901, by R. D. Oldham. The delay was partly due to low magnification and inadequate damping in the early instruments, and partly to the fact that earthquakes send out other waves that travel over the surface, and at large distances are much larger than those predicted by Poisson. People tried to identify the first movement with the longitudinal wave and the large later movement with the transverse wave, until Oldham noticed that in an Assam earthquake the large waves travelled with nearly uniform velocity over the surface. A bodily wave in a uniform solid would have travel times proportional to the chord. The travel time of the first movement actually increased less rapidly than the chord, and Oldham inferred that this was a body wave with the velocity increasing with depth. With this hint to guide him, Oldham was able to detect a sharp movement between the first movement and the large waves, with travel times roughly proportional to those of the first movement, and he identified this with Poisson's transverse wave. The symbols P and S were given by von dem Borne and Wiechert to the longitudinal and transverse waves; they stood originally for *primary* and *secondary*, but H. H. Turner remarked that they could equally well stand for *push* and *shake*—though P is as often as not a *pull*.

The next great advance was also made by Oldham. He found that P was well observed up to a distance of about 100° (measured as an angle subtended at the centre of the Earth) but beyond that it disappeared. It appeared again at about 140°, but so much later that it was clear that there must be a central region where the velocity had dropped very substantially. Up to that time no direct method was known for finding the distribution of velocity from the travel times; it was necessary to use a laborious method of trial and error, but a direct method was found by Herglotz and Wiechert. The questions introduced by the discovery of the core were discussed in a series of brilliant papers by Gutenberg. He predicted other waves produced when P and S in the shell strike the core boundary, and found several of them clearly detectable in the records. They included some reflections at the inner surface of the core; reflections at the outer surface of the core were not found till later, chiefly because they arrived at times when they were overlaid by the much larger surface waves; they had to wait for instruments specially sensitive to short-period movements and for the study of deep-focus earthquakes, in which the surface waves are small. But these reflections have now been observed with such accuracy that they can give the radius of the core to something like 4 km. Incidentally they are so strong that the transition from shell to core cannot be more than a few kilometres thick.

All the waves that Gutenberg found through the core had been of P type in the core. S in the shell is partly reflected at the core boundary and partly transmitted as P. The core seems unable to transmit S waves, which strongly indicates that it is liquid. Other interpretations have been tried, but none is satisfactory.

There was no sign in the seismic data of any marked change of properties where the boundary of the core would be according to Wiechert's model. The radius of the core is 0·545a and therefore much less than Wiechert's estimate. Wiechert had in fact not allowed for the effect of compressibility in increasing the densities, having no data to do so. Knowledge of the velocities of elastic waves has made this possible. Detailed study by Bullen of the effect of compressibility, combined with what we know of the mass and moments of inertia, has told us a lot about the distribution of density all the way to the centre, though some minor features are still open to discussion.

The evidence so far has dealt only with mechanical properties, namely density, compressibility, and rigidity. We should like to know what the Earth is really made of, but direct chemical analysis is possible only for surface rocks. The elastic properties of actual rock specimens in the laboratory suggest that far the most likely material for most of the shell below a depth of 30 km. or so (less under the oceans) is olivine $(Mg, Fe^{II})_2\ SiO_4$, possibly with a crystallographic change

about 300 km. down. The core had been generally supposed to be iron, but a startling alternative was proposed a few years ago by W. H. Ramsey. There has been strong evidence for a long time that certain stars have densities many times greater than that of any known solid, and this has been explained in terms of what is called degeneracy. When atoms are sufficiently tightly pressed together, the outer electrons become too close together for comfort and wander through the whole structure. Much work has been done on the theory of this state. Ramsey's contribution is to suggest that the first stage of it can arise in a silicate crystal at planetary pressures ; olivine could then acquire the properties of a metal, and Ramsey maintains that the core is not iron but olivine in this new state. It seems to me that this is a problem worth the attention of workers on crystal structure. It should, I think, be possible to work out the behaviour of a silicate at these high pressures and find out at what pressure the change would take place. Perhaps even the density of the new modification could be predicted. If the results agree with what we know of the densities at the core boundary the theory would be practically irresistible. Personally I prefer to await such a test before expressing an opinion. Ramsey has appealed also to evidence from cosmical chemistry, and his opponents have quoted similar evidence against him. But I should mention that by discussing analogous behaviour of hydrogen (following work of Kronig and others) Ramsey has succeeded in explaining the densities and figures of Jupiter and Saturn. The density of Saturn is well under 1, and it was difficult to think of any substance that would have so low a density at the high pressures concerned. These planets appear to consist mostly of hydrogen, with the central parts in a metallic state similar to that of sodium at ordinary pressures.

Another contradiction that was unresolved in 1900 concerned the Earth's thermal state. If the rate of increase of temperature with depth observed near the surface persisted, melting temperatures would be reached at a depth of about 50 km. Consequently it was generally believed that apart from a solid crust with a thickness of this order the Earth must be fluid. This is stated as a fact in some quite recent popular books. Kelvin had given strong arguments against this belief. He had actually shown that on an Earth with such a constitution there would be no oceanic tides ; the crust would bend so freely under tidal forces that the ocean surface could rise no more than its bottom would, and tidal observations measure only the difference. Means are available for measuring the tidal straining of the body of the Earth, and the results are in good agreement with theory based on present knowledge of the Earth's mechanical properties.

The rate of increase of temperature as we go down is mostly maintained by radioactivity in the upper layers. Radioactivity must be practically confined to a thickness of 15 km. or so, otherwise the rate of outflow of heat, which we can measure, would be too large. Below this level the temperature gradient must be much less than near the surface. With modern estimates of the age, again, there has been time for considerable cooling to a depth of about 700 km. This is also near the depth where the deepest earthquakes occur. Fractures could not occur in a true fluid, but in an ordinary solid a secular change leads to an accumulation of stress until the stress-differences reach the strength of the material, when fracture takes place. Deep-focus earthquakes therefore indicate that the Earth is an ordinary solid to a depth of at least a tenth of the radius. The old idea of a fluid Earth, except for a thin crust, still survives in the various convection theories, which suppose that the material of the shell can flow continuously under a small stress. I cannot go into detail, but one comment is that a great deal of work has now been done on the plasticity of solids, and all the evidence indicates that flow is negligible except near a particular value of the stress-difference ; there is no positive evidence that the mathematics of viscous flow, in which the rate of change of shape is supposed proportional to the stress-difference, is ever applicable to materials with rigidities similar to those we find in the Earth. The direct evidence, such as rise and fall of the land surface, the distribution of gravity, and mountain

formation, is in better agreement with the hypothesis that they are due to temperature changes, mainly cooling, in an ordinary solid.

Igneous phenomena must be regarded as exceptional ; the hypothesis of a permanently liquid layer, such as could be regarded as a possible source of liquid silicates rising to the surface, would reinstate the difficulty of accounting for the tides.

I have confined myself to internal geophysics and ignored the ocean, the atmosphere, and terrestrial magnetism. Other speakers will be attending to some of the associated problems. Until recent times terrestrial magnetism has been discussed in relation to the atmosphere and solar effects, and the results for the most part have been independent of those found by investigation of the interior of the Earth by mechanical and thermal methods. This state of affairs seems to be coming to an end. The first sign, I think, was when Chapman, Price, and Lahiri found a rapid increase of electrical conductivity at a depth of about 400 km., which is in a range of depth where there is a specially rapid increase of the velocities of seismic waves and probably of density, probably due to a crystallographic modification. At present Bullard and others are working on the hypothesis that changes of the Earth's magnetic field are due to electrical currents in the liquid core. It seems possible that the variation of electrical properties with depth may help to test our inferences on the temperature distribution.

Scientific method is essentially a matter of successive approximation ; we cannot avoid guesswork entirely, but the more data we have and the more carefully we analyse them, the narrower the range of permissible guessing becomes. The chief change since 1900 is the great increase in accuracy ; in seismology, for instance, we worry about discrepancies of 2 to 4 seconds in total travel times running up to 20 minutes and more, whereas the best times available in 1910 had errors up to 30 seconds. The temperature distribution is perhaps the most doubtful part remaining, but theoretical and laboratory work are together giving clues to the thermal, electrical, and mechanical properties of matter at high temperatures and pressures. So far the results are not so consistent as we should like ; arguments can be given for supposing that the temperature at the core boundary is anything from 2,000° to 5,000°, but at least the problem is being tackled.

Geophysics originally arose from the need to know the size of the Earth in order to co-ordinate survey measurements with differences of latitude and longitude ; from the use of the mariners' compass ; and from the destructive effects of earthquakes. The further developments I have discussed have been undertaken almost independently of economic application, but when they reached sufficient accuracy it was seen that they were applicable to ordinary geology, and especially to the detection of strata bearing oil and water. Nearly all the oil used in industry comes from sources detected by methods originally devised for the study of natural earthquakes. In fact now when people speak of geophysics it often turns out that they mean geophysical prospecting and nothing else. This has reacted on pure geophysics, since the techniques devised for geophysical prospecting are also being used for geophysical survey where there is no immediate economic application ; and much important information on the structure at small depths has been acquired in this way.

I cannot close without taking an opportunity of asking physicists (of all branches) to pay more attention to the presentation of their data so that they will be of most use to other workers. I refer especially to the practice of giving a quantitative result with no estimate of uncertainty, or with one that represents only the writer's personal impression. Science advances by comparison of results found by different methods. If such results are to be compared, it is first of all necessary to know whether any discrepancy is large enough to be an improbable result of the ordinary random scatter of the observations ; and this standard is given by the standard error computed by the method of least squares and in no other way. If an author suspects systematic error, there is no reason why he should not say so ; but it is additional information to the standard error and not a substitute for it—and

authors sometimes suspect systematic errors that are not there. (The adopted standard of reference for gravity is wrong for this reason.) Statements of uncertainty in my own experience have ranged from a tenth to ten times the standard error, according to the authors' optimism or pessimism, which is not even a constant for one author. An overestimate of uncertainty may lead to the overlooking of discoveries of the first importance. An underestimate may lead to much waste of time through suggesting discrepancies that never had any basis. I do not always agree with Sir Ronald Fisher, but it is due to him that the standard of presentation of results in agriculture is better than in any of the so-called exact sciences; and this is a state of affairs that physicists should cease to tolerate.

I think that what I have said is specially appropriate to a meeting of the British Association. The Association's long support of seismology was described at length in Dr. Stoneley's presidential address to the International Seismological Association in 1951 : it played an essential part in inaugurating the international collection and analysis of data from which many modern advances have proceeded. The Joint Committee of Sections A and C on the Thermal Conductivities of Rocks, set up in 1935, began a new era in the study of the Earth's thermal state, in which physicists, geologists and commercial companies have most effectively co-operated.

A SUGGESTION AND SOME CONSEQUENCES

SOME REFLECTIONS ON LIEBIG'S CALL TO BRITISH CHEMISTS AT THE LIVERPOOL MEETING IN 1837, TOGETHER WITH OBSERVATIONS ON THE NATURE OF CITY SMOKE

Address by

PROF. G. R. CLEMO, F.R.S.

PRESIDENT OF SECTION B

WHEN you did me the honour of inviting me to be President of Section B this year, I looked up the record of the proceedings of the first meeting of the Association in Liverpool in 1837 and was quite taken aback to find that Faraday was its President on that occasion !

The Secretary informed me however that, so far as is known, Faraday did not attend that meeting, although he had attended others and that the function of the President then, and for many years, was to act as chairman of committees and arrange for the orderly presentation of papers at the meeting.

This most desirable procedure was continued until the 1858 Meeting at Leeds when Sir John Herschel is stated to have opened the meeting of Section B with an introductory address of such singular interest that the precedent has been followed ever since.

In 1837 Victoria had just come to the throne, cholera epidemics were common and claimed over 30,000 victims yearly in this country at that time, and it was only in the next year that the first steamship crossed the Atlantic. A remarkable contrast to 1953, but no more so than that presented by the position of chemistry at the corresponding dates.

This country was poor in 1837 from the French wars, and the rapid growth of the industrial population, with the need for cheap food to keep the cost of production low, created a lively interest in improving agriculture. An enduring problem.

Although, as stated, Faraday does not appear to have attended the meeting, Dalton did and contributed a paper whose conclusion was that the assistance of plants for the purification of the atmosphere was not necessary !

The outstanding personality present, however, was Liebig who, having already transformed the technique of organic analysis, was led to study chemical changes during growth and decay in the plant world, and thus to change the practice of manuring. At this meeting he stated :—

> 'There are many bodies similar to urea and allantoin, all of which will probably, at a future period, be produced by artificial means ; but in order to arrive at this, the final object of investigation in organic chemistry, a great deal of labour, and that labour of a combined nature, will be required. I am certain that this object will be attained. Organic chemistry has made its first step, and already its field has been extended to a very surprising degree. We meet every day with new and unexpected discoveries. It is, however, remarkable, that in the country in which I now am, whose hospitality I shall never cease to remember, organic chemistry is only commencing to take root. We live in a time when the slightest exertion leads to valuable results, and, if we consider the immense influence which organic chemistry exercises over medicine, manufactures, and over common life, we must be sensible that there is at present no problem more important to mankind than the prosecution of the objects which organic chemistry contemplates. I trust that English men of science will participate in the general

movement, and unite their efforts to those of the chemists of the Continent, to further the advance of a science which, when taken in connection with the researches in physiology, both animal and vegetable, which have been so successfully prosecuted in this country, may be expected to afford us the most important and novel conclusions respecting the functions of organisation.'

I propose, therefore, to venture on a few words of introduction to the proper business we now have in hand by summarising briefly the consequences of Liebig's challenge and then concluding with some remarks on two of the research topics upon which we are at present engaged, and which I think fit well into my theme.

As a result of Liebig's visit he was invited to write a report on the application of organic chemistry to agriculture and physiology. This, on publication in 1840, made such a great impression that he was invited to stay in 1842 with the Prime Minister Peel. The outcome was a movement for the establishment of a college where chemistry could be pursued along Liebig's lines, and the Prince Consort becoming interested, money was soon collected and Liebig asked to recommend a Professor to direct the new college. Of his three recommendations, Fresenius, Will and Hofmann, it was, I think fortunate that the first two refused.

Hofmann in London

Hofmann had studied law, mathematics and physics, but responded to Liebig's inspiration and the conception of science as 'an imaginative guide to spiritual and material betterment' with the result that he became one of the leading organic chemists for many years around the middle of last century. The Royal College started in a house in Hanover Square in 1845, with 26 students, the premises being extended the next year. The first research report in 1849 began with a paper by Warren de la Rue on the colouring matter of cochineal, another was by Nicholson and showed that the stimulating extracts of tea and coffee were identical (caffeine appears to have been first isolated in 1820), and the last by C. V. Mansfield described the fractional distillation of coal tar products, with the isolation therefrom for the first time of benzene. In the next report in 1851, Hofmann wrote 'We believe that tomorrow or the next day someone may discover a method of producing from coal tar the beautiful colour of madder, or the valuable remedies of quinine and morphine.' Truly prophetic, and from then to the present time the chemistry of natural products has been a prominent feature of British organic chemistry. W. H. Perkin entered the college in 1853 when 15, and was so influenced by Hofmann's 1851 remarks on quinine that mauve was discovered in 1856! A factory was built for its production in 1857 and the pioneering venture guided and driven by Perkin's genius prospered greatly. Probably his greatest success was his synthesis of alizarin in 1869, thus fulfilling one of Hofmann's predictions. He sold his six acre factory, in which he had made eight dyes, in 1873, for £100,000, and retired to a life of research and leisure in which music was a real passion. This was a truly magnificent start to the organic chemical industry, the first to be based on research although, alas by only one worker, and England was soon to lose the lead, whilst the dyestuff industry became firmly established and prospered greatly in Germany. Amongst the reasons for this was the lack of vision and leadership in our old universities, with the consequent neglect of chemical research in this country. Hofmann's own experience illustrated this point, for although his great work was only costing about £1,200 a year, it did not fill the bins with grain overnight, and in consequence the Governing Body, including 37 landed peers of the realm, lost interest, and the College was soon in financial difficulties. It was only saved by being merged in 1853 as the Chemistry Department of the Royal School of Mines but in consequence lost its character as a purely research institution. Hofmann accepted a call to Berlin in 1865, and nine years were to elapse before the first chair of organic chemistry was instituted in this country.

Manchester's early contribution

At the time our civic universities were just struggling to be born and the old universities were dominated by theology,

classics and mathematics. The appointment of Roscoe, who had been a pupil of Bunsen, to the chair of chemistry at Owen's College in 1857, following Frankland's departure for South Kensington, was the turning point in changing this attitude. On his resignation from the chair thirty years later, he wrote a report from which the following points are taken. In 1857 Owen's College had 34 students, 15 of whom worked in the chemistry laboratory. The poor opinion held by the Governing Body, of Chemistry was shown by the fact that Roscoe's salary was only half as much as that of the professors of classics and mathematics at the time. He was a great teacher however : ' It is in the laboratory and there alone that chemistry can be taught ' ; and from the beginning he emphasised the value of research. ' The aim of a school of chemistry is not limited to training technologists. Its highest function is to guide students to original scientific investigation.' During most of Roscoe's period, his students took the London examinations, including the Associateship of the Institute of Chemistry, but by the time he left there were 120 workers in the chemistry laboratory and some 30 of his students were holding, or were soon to hold, responsible teaching posts elsewhere. Amongst those who held chairs were P. P. Bedson (Newcastle), T. Carnelley (Dundee), J. B. Cohen, F.R.S. (Leeds), W. Dittmar, F.R.S. (Glasgow), A. Smithells, F.R.S. (Leeds ; it is interesting to find amongst the latter's published work one on the fluorine compounds of uranium !), T. E. Thorpe, F.R.S. (South Kensington), W. Carleton Williams (Sheffield), and C. R. A. Wright, F.R.S. (St. Mary's Hospital School, London). Roscoe had thus laid a magnificent foundation, although it must be said that many of the above torch bearers came under the influence of Kekulé at Bonn. When Dittmar left for Glasgow, he recommended C. Schorlemmer, a young chemist from Darmstadt as an assistant, who, after serving as demonstrator for thirteen years, was appointed to the first chair of organic chemistry in this country in 1874, which continued to be the only one until 1887. His 37 original papers between 1860 and 1881 included many of interest on the paraffins, and with collaborators he published 16 others, mainly on dyes such as aurin and also on suberone.

The development generally in this country

The position in this country, however, compared very unfavourably with that in Germany during the same period.

During this period, bits and pieces of organic research were also done at various centres in this country, but nowhere was there any school of organic chemistry comparable with Baeyer's at Munich, which had already done the classical work on indigo.

A brief review of these bits and pieces, however, is not without interest, and the following remarks are based mainly on papers published in the *Journal of the Chemical Society*, which, although founded in 1841, had no regular publication until Volume I appeared in 1848. Previously a few papers had been published in the *Phil. Trans.* ; thus Parts I to IV on ' The constitution of guaiacum resins ' by Johnston of Durham appeared in 1839–40. It is an interesting coincidence that R. D. Haworth's work on the constitution of guaiacum resins, Parts I, II and III came from Newcastle between 1932 and 1935.

The period up to 1849 is mainly the record of the work of the Hofmann school, particularly on the ammonium bases. The precursor of Frankland's radical work appeared in 1848 in a joint paper with Kolbe from Marburg on ' The action of potassium on ethyl cyanide.'

The 50's also reflect the work inspired by Hofmann, and continued to show the importance attached to natural products. Thus Nicholson and F. A. Abel analysed strychnine and gave it the empirical formula $C_{42}H_{22}O_4N_2$. It is indeed fortunate, in view of the structural problem subsequently presented by this alkaloid, that its formula was based on the atomic weights of 6 and 8 for carbon and oxygen respectively ! In passing, mention may be made that the correct atomic weights of 12 and 16 for these two elements were only accepted generally after Lothar Meyer in ' Modern Theories of Chemistry ' had expounded the views which Cannizzaro had put forward at the Carlsruhe Congress called in 1860 to discuss the vexed question of atomic weights. It

should be mentioned, however, that Odling in a paper in 1858 gave good reasons for C 12 and O 16, and W. H. Perkin in his 1861 paper on mauve used the correct atomic weights. In the 50's Frankland published a series of papers on the radicals, in the first of which he refers to ethyl and methyl. This appeared in 1850, whilst he held the chair of chemistry at the College of Engineers at Putney and before he proceeded to the chair at Owen's in 1851.

His methyl and ethyl refer of course mainly to ethane and butane and over fifty years were to elapse before the radicals were set free and many more before the realisation of their great importance in chemistry and industry generally.

In 1852 the classical work by Williamson at University College on etherification appeared and propylene was first described at the Royal College of Chemistry the previous year as a new hydrocarbon C_6H_6. Incidentally ethylene which had been prepared towards the end of the last century by the Dutch chemists was described by Guthrie in 1861 as C_4H_4 and acetylene by Berthelot in 1863 as C_4H_2. I wonder how many final honours chemistry students to-day could give a good account of the sound chemical proof for the ethylenic double bond as advanced in the early 60's mainly by Erlenmeyer senr. and described in Volume I of the first edition of Roscoe and Schorlemmer. [Certain it is that if a student of the 60's had stated that ethylene would be used for making service pipes for laboratories he would have received scant recognition for the statement.] This conception of course, depended on the enunciation of the fundamental idea of the quadrivalency of carbon which was arrived at independently and announced almost simultaneously by Couper from Edinburgh and Kekulé in 1858. Apparently Couper reached this conclusion first but his subsequent ill health prevented him from elaborating the idea and to-day the major credit is ascribed to Kekulé.

In 1857 a name appeared in our Journal for the first time which was to dominate British chemistry for the next seventy years. A. H. Church and W. H. Perkin senr. described some new colouring matters ; this was not mauve as might be expected (which had been discovered the previous year and which was mentioned by the President of the Chemical Society at the Annual General Meeting in 1857), but referred to the action of nitrous acid on α-naphthylamine. Great interest attaches to the colouring matter so formed, since it was the first representative of the large group of azo dyes ever made. Its true nature was not recognised, however, but when examined seven years later patents were taken out, although as is well known Griess only announced his diazo reaction in 1867.

The 60's still reflected largely the work of Hofmann's school. Many alkaloids were described, several analysed and their empirical formula determined, as for example, sparteine by Mills in Hofmann's laboratory, of berberine by J. Dyson Perrins and of narcotine by Matthiessen, who was a lecturer at St. Mary's Hospital school, together with G. C. Foster who was professor of physics at University College.

Perkin described his work on mauve for the first time in 1861. This discovery is all too often thought to be a blind shot in the dark, but remembering that it took place nine years before Kekulé had formulated the ring structure of benzene, Perkin's equation

$$2(C_{10}H_{13}N) + O_3 = C_{20}H_{24}O_2N_2 + H_2O$$

Allyl-toluidine — Quinine

showed considerable vision at that time.

In 1868 Perkin's well-known coumarin synthesis appeared. Of outstanding interest also was Crum Brown's paper in 1866 on the theory of isomeric compounds, and one of our early biochemists Thudichum (St. Thomas' Hospital) published a paper on the constitution and reactions of tyrosine. This makes curious reading to-day, tyrosine being given as

$$N''C.CH_2.CH_2CH_2.CH_2.CH_2.CO.CO.C\overset{O}{\underset{H}{}}.$$

More will be said on this subject later.

Guthrie described several other olefines about this time and Anderson of Glasgow in 1863 published an account of anthracene, still written as $C_{28}H_{10}$.

The crowning glory of this period, however, was the enunciation of the tetrahedral nature of the four valencies of carbon by Van't Hoff at Utrecht, and le Bel at Paris in 1874, on which basis the

whole vast edifice of structural organic chemistry was to be built.

The 70's began with an account of Perkin's outstanding work on alizarin, followed by a paper on anthrapurpurin in 1873, in which year, as stated already, he quit the chemical industry. In 1872 the dynamic personality of H. E. Armstrong appears in the arena for the first time with a paper on phenolsulphonic acid, whilst C. R. A. Wright, now at St. Mary's Hospital school, contributed numerous papers at this period on alkaloidal topics, including narcotine, contarnine, morphine, codeine, the aconites I and II, veratrum I and II and also a series of five papers on isomeric terpenes. How many organic chemists of to-day have even heard of his name ?

Another familiar name appeared for the first time in the 70's. Thus in 1877 Tilden recorded the beginning of his work on the action of NOCl on the terpenes whilst still a master at Clifton College. In 1880 he went to the first chair of chemistry at Mason College.

In 1879 the first carbohydrate paper on the transformation products of starch by C. O'Sullivan appeared in the Journal. He was trained in Hofmann's laboratory, went with him to Berlin in 1865, returned in 1866 to become second chemist to Bass & Co. at Burton and after doing much valuable work was awarded the Longstaff Medal in 1884 and became a Fellow of the Royal Society in 1885.

The output of research in the 80's increased considerably, and the outstanding development of this period was that of W. H. Perkin junr. on small carbon rings in which the aliphatic and aromatic series were bridged by various C_3, C_4, C_5 and C_6 membered rings. The work which began in Baeyer's laboratory in Munich appeared in several papers in the Journal and the story was told by Perkin himself in the first Pedler Lecture in 1927. This work was based largely on the action of sodium on esters, and would appear to have stemmed from the 1878 paper by W. R. Hodgkinson of the Royal College of Chemistry on the action of sodium on benzyl esters, to be followed two years later by a joint paper by Perkin and Hodgkinson on the action of sodium on phenylacetate.

In 1883 the brother, A. G. Perkin, who later attained eminence for his work on natural colouring matters at Leeds, published his first paper on diphenylene. Other items of note were Japp's work from South Kensington on phenanthrene (the Japp-Klingemann reaction came from the same centre in 1888) and the emergence of two chemists who were later to become well known in the dyestuffs field, a paper by Meldola appearing in 1881 and one by A. J. Green on primuline in 1889, in which he made the important discovery that azo dyes could be developed on the fibre. In 1885 E. Frankland discussed the topic of chemical changes in their relation to micro-organisms, and thus opened a chapter which is now of the greatest importance in chemistry. In this decade papers appeared from several provincial centres, e.g. T. C. Carnelley (Dundee), J. J. Dobbie and others (Bangor), T. W. James (Cardiff), O. Masson and Alex Smith (Edinburgh), T. Purdie (St. Andrews), W. A. Shenstone (Clifton College School), W. A. Tilden (Birmingham), A. Tribe (Dulwich College), and so on. It is to be noticed that although much good work was coming from the provinces, from the hospital laboratories and from public schools there was little sign of life at Cambridge and none yet at Oxford. At Cambridge, Stewart who had worked at Strasbourg, published two minor papers in 1886, and in 1887 Ruhemann (recommended to Dewar by Kekulé) published a paper on the pyridine derivatives from citric acid. In 1887 W. H. Perkin junr. went to the Heriot-Watt College and his first paper on berberine came from Edinburgh, but he left to fill the chair of organic chemistry in Manchester in 1892. Even now, however, there was not a single flourishing school of organic research in Britain whereas on the Continent those of Baeyer with Dieckmann and Thiele (Munich), E. Fischer and Liebermann (Berlin), V. Meyer with Gattermann and Knoevenagel (Heidelburg), Claisen (Aachen), Fittig (Strasbourg), Friedlander (Zürich), Hantzsch (Würzburg), Kekulé (to 1896 at Bonn), Knorr (Jena), Lothar Meyer (Tübingen), Wallach (Göttingen), Wislicenus (Leipzig), were producing work of the highest value.

The very interesting accounts of British

Schools of Chemistry now being published by the Royal Institute of Chemistry paint, in my opinion, a rather too favourable picture of the position last century.

In the 90's much work of a varied character was published, and the flag even began to flicker at Oxford, with J. E. Marsh's first paper on camphor. Thus structural organic chemistry at Oxford had a humble beginning. Many new authors who subsequently became eminent appeared in the chemical literature for the first time. J. C. Cain and J. B. Cohen (both at Manchester), F. D. Chattaway (St. Bart's Hospital, on quinones), H. J. H. Fenton (Cambridge), P. F. Frankland (Dundee), F. S. Kipping with W. H. Perkin junr. (at the Heriot-Watt), J. J. Sudborough (Nottingham), and J. N. Collie, C. F. Cross and E. J. Bevan, M. O. Forster, T. A. Henry, J. T. Hewitt, T. M. Lowry, G. T. Morgan, W. J. Pope, jointly with H. E. Armstrong from 1891 (all in London). Alas, all but Henry and Hewitt have passed on. A further name must be added to the fairly long list just given, and that is A. Lapworth, who began publishing on the terpenes from the City and Guilds Technical College with Armstrong, and whose mind had already as early as 1898 begun to work along theoretical lines as shown by his paper on 'A possible basis of generalisation of intramolecular changes in organic compounds' and in which he refers to H. E. Armstrong's 1887 paper on substitution in benzene. This influence may well have been H. E. Armstrong's greatest single contribution to chemistry.

This century

The significant feature of the 1900–9 period was the development of Perkin's Manchester school, which produced many outstanding leaders, including J. F. Thorpe, C. Weizmann, later to become the statesman, W. N. Haworth, J. L. Simonsen, and above all, Robert Robinson, who was to dominate organic chemistry from then to the present time. This school has had a profound and lasting influence on British organic chemistry. The only other school of chemistry in Britain at the time was Ramsay's at University College on the sister section of inorganic chemistry.

Much first-rate work, however, came at this time also from various technical colleges, e.g. G. G. Henderson (Glasgow Technical College), T. P. Hilditch (East Ham Technical College), W. H. Mills (Northern Polytechnic) and R. H. Pickard (Blackburn Technical College).

Another great feature of this time was the start of the brilliant work of F. Gowland Hopkins at Cambridge on the vitamins and hormones, which was to have such a profound influence on organic chemistry and biochemistry in general and on nutrition in particular. H. O. Jones also began his most promising work at Cambridge in 1905, only to have it cut short by his tragic death in the Alps on his honeymoon in 1912. Titherley was publishing from Liverpool, Kipping produced his first paper on organic silicon compounds in 1907, and probably had no idea of their coming great industrial importance. Even the Sunday press now extol the merit of the silicones, with their great resistance to heat and cold and their applications in paints and varnishes and as jointing and insulating materials for jet engines. Pickard's paper in 1908 appears to be the first in the Journal on the sterols, a topic which after 1930 dominated organic chemistry for many years. R. Robinson's first Journal paper was in 1907 on brazilin, whose synthesis still baffles even his genius (small items of his work appeared in 'Proceedings,' 1905–6), and his contributions to date in settling the architecture of nature's molecules have been truly amazing. Harmine, morphine, strychnine, emetine, penicillin etc. have had their structures wholly or mainly unravelled by his brilliant work and insight. In addition he has done great synthetic work in the sterol and many other fields.

Even so, however, many great molecular peaks await scaling by the synthetic chemist.

Most of you will be familiar with the main developments in organic chemistry in this country since 1910, and time only permits of the mention of some of the main lines of advance since then. In 1912 Perkin moved to Oxford, won the 'Battle of the Parks' and following the building of the Dyson Perrins Laboratory, chemistry came to occupy its rightful place in that ancient seat of learning.

The First World War found this country with a most insignificant organic chemistry industry, a revelation that shocked the country into action, with the result that to-day it will bear fair comparison with that of any other country, and many great advances have emanated from its research laboratories since 1920. Some of these will be mentioned shortly.

Amongst the major items of academic advance since 1910, first mention may be given to R. Robinson's classical 1917 paper from Liverpool on the synthesis of tropinone, and thence of tropine. In this he began to show for the first time the way his mind was beginning to work on the structural relationships running through natural products. This subject has now assumed such importance and been of such great value in the elucidation of structural problems, as for example strychnine, emetine etc., that when planning this morning's programme it seemed only natural that the author of this great development should be invited to give an outline of its present position. I was indeed gratified when he accepted, but alas the American Chemical Society has decreed that he should not attend and he is now nearing Chicago to receive the Priestley Medal. He has very kindly written his paper however, and this will be presented as the next item on our programme.

Another striking achievement that this country can claim is the elucidation of the nature of the tropolones, following Dewar's brilliant conception of the seven membered ring. Thus two of the five big advances concerning carbocyclic ring systems are to the credit of this country.

The discovery of the electron by J. J. Thomson in 1897 furnished the basis for Lapworth's theoretical musings in 1898 and led ultimately to the present-day electronic theory of organic reactions. These ideas began to take definite shape in Lapworth's writings soon after 1920, but his reasoning was not always easy to follow and Robinson showed his great versatility by helping to clarify and develop them. Ingold and his colleagues have vastly extended the subject in recent years and we are indeed glad Professor Hughes will wind up this morning's session with an account of this great development. One striking consequence of this advance is reflected in the changed nature of many organic papers, whose authors try to give a theoretical explanation for every experimental result obtained.

In addition to the above major trends for which this country can claim great credit the fascinating sterol story only became definite when the ideas of King and Rosenheim in 1932 illumined the perplexing structural problem posed by the extensive experimental work of Wieland and Windaus and thereby led to that spectacular advance which embraced the D vitamins and has continued without a break to the present time as instanced by the story of the hormone cortisone. Only recently the 9α-chloro derivative has been shown to be five times as active as cortisone. The sterol story is a fine example of the aid which the modern physical methods using X-rays, U.V. and latterly I.R. spectra bring to the elucidation of structural problems. Mention should be made that most of the recent advances in organic chemistry would not have been possible but for the replacement of Liebig's original analytical methods by present-day micro-techniques.

The not unrelated story of the carcinogenic hydrocarbons has also assumed great importance since benzpyrene was shown to be the causative agent in tar cancer by Kennaway and Cook in 1932 and this talk will end with a preliminary account of some of the results obtained in an examination of city smoke.

Of the greatest value also in organic chemistry generally and especially in the modern work on the proteins has been the help afforded by chromatography. This is one of the discoveries made elsewhere long ago which has been effectively applied in this country as a result of the well-known work of Martin and Synge on partition chromatography.

On the vast subject of nutrition in the plant and animal kingdoms respectively this country has also played a notable part in the work on the vitamins, whether A, the B's, C, the D's and the rest of them, including the isolation of the important cobalt containing B_{12} as well as in the hormones such as thyroxine and the recent striking work on insulin.

Could Liebig return to-day he would

surely exclaim that this country ' has surpassed all his notions.' The great advances in industrial organic chemistry are almost too numerous to mention but outstanding amongst the discoveries are Caledon jade green and the phthalocyanines amongst the colouring matters, and perspex, polythene, terylene and ardil amongst the plastics and fibres.

Carothers began ' to synthesise giant molecules by strictly rational methods ' in 1927, and yet the American chemists missed terylene and they are now watching closely our work in the plastics field.

The selective weed killers and insecticides like gammexane have also been largely developed here. On entering a seedsman's shop with its shelves stacked with tins of bio, thio etc., it is no surprise to learn that the use of organic pesticides increased in the United States by 250 times between 1941 and 1951. Even so, Ordish in *Untaken Harvest* published in 1952, claims that in 1947 the produce of 2 million acres and the skilled work of 51,000 men was wasted in this country, and the tragic loss from foot and mouth disease will be familiar to all. An interesting departure in the practice of manuring has been recently introduced whereby urea is absorbed directly by the leaves of the plant. Incidentally this key organic substance is now stated to be one of our big dollar-earning exports.

Concurrently with the dyestuff industry and often using its by-products, the drug industry began early to develop in Germany, antipyrine being introduced in 1886, aspirin in 1899, and it received a vast impetus when Ehrlich laid the foundation of modern chemotherapy. It has now assumed colossal proportions, thus the U.S. in 1935 spent 10 million dollars rising to 100 million in 1951 on drug research, the sales being 291 and 1,424 million dollars respectively for the same years. It is not surprising, therefore, that they have made rather startling discoveries in several lines and now even use drugs fairly generally as lie detectors ! This country, however, is in no way behind and has made so many notable contributions that Monday's session which Professor Robertson has kindly organised will be devoted to antibiotics and chemotherapy generally.

Good things, however, even more than bad tend to be abused. This applies with special force to drugs, since good health is our most priceless possession and its pursuit by the ailing is apt to lead to excesses. Thus it has been stated recently that no fewer than 10 million aspirin tablets were used in this country last year.

The critics of the use of anaesthetics and chemotherapy generally, however, might well reflect on the old days when, for instance, a brain operation could only be done by rubbing a hole in the skull with a stone.

The tremendous industrial developments touched on above are due to the building up of big research staffs by industry and their beneficial interplay with the academic staffs doing fundamental research in the universities. This also has its dangers and there is widespread concern and almost alarm at the dearth of science teachers in our schools. One set of figures is enough. For the session 1951–2 of the 482 honours science graduates from Oxford, only 17 became schoolmasters and all in public schools. Undoubtedly the relative salaries paid by industry and the teaching profession have a good deal to do with this and the following figures speak for themselves.

	Average Salary	
Age	*Teaching* (Grammar Schools)	*Industry* (from Institute of Chemistry statistics)
32	£651	£1,004
35	£705	£1,244
46	£726	£1,651
	Special responsibility £50–£100	Many industrial salaries are increased by 20 per cent.

Pensions also favour considerably the industrial chemist. In addition, the satisfaction obtained from a research or manufacturing career producing results of far-reaching value also plays an important part.

A further serious consequence of this lack of science teachers is that there appear to be very few science ' Heads ' in our grammar schools. Thus for 25 such schools in Newcastle and Northumberland no less than 10 ' Heads ' graduated in classics, 3 in

English, 3 in history, 2 in mathematics, 2 in physics, 1 in chemistry, 1 in biology, etc.

It is not easy to suggest a solution to this serious problem and yet one must be found soon. Can the universities help by encouraging research by science masters in schools? Are any schools doing research, as Clifton and Dulwich did to their great credit last century?

A glance ahead

Now to look forward for a few moments. In spite of all the remarkable advances, particularly in structural and synthetic organic chemistry, Nature still does most of its great work behind its curtain. Our ignorance here is truly great, although considerable light has recently been shed on both the carbon and nitrogen cycles. The former used to refer to the intake of carbon dioxide by plants and its metabolism through many intermediate products and their final conversion to CO_2 again. Now, however, a quite new aspect of the cycle has been claimed to be of the profoundest importance, namely that of $^{13}C \rightarrow {}^{13}N \rightarrow {}^{14}C \rightarrow {}^{14}N \rightarrow {}^{15}O \rightarrow {}^{15}N \rightarrow {}^{12}C$ which needs apparently 5–6 million years for its completion and is held to be the essential catalyst in the process whereby in the sun 592·2 million tons of hydrogen are converted to 588 million tons of helium per second with the conversion of the balance of 4·2 million tons to produce the stupendous energy emitted by the sun and which keeps all life as we know it functioning. If the nitrogen-fixing bacteria in certain plants could function slightly better there would be much less need for synthetic nitrogen fertilisers. The study of chemical microbiology is probably our greatest need at the present time.

To give a very few instances of our ignorance, we scarcely know how the vitamins function in nutrition; not even the simplest C which is essential for man and some other animals but not for others; or how the black pigment of our hair and skin is formed, or why the plant auxins make the hop climb the stick in one direction and the kidney bean in the other, or to prevent let alone cure cancer.

In the elucidation of such problems it is not too much to claim that the discovery of the isotopes, particularly of carbon and nitrogen within the last decade will play a decisive role. Their limited availability here is not so much a handicap as their great cost, as shown by the following figures:—

$Ba^{14}CO_3$ with 1 m. C. of activity in 40-45 mg., £12;
$Ba^{13}CO_3$ 70 per cent. enriched per gm. of ^{13}C, £40;
$^{15}NH_4NO_3$ 35 per cent. enriched per gm. of ^{15}N, £80;
and $H_2{}^{18}O$ 5 per cent. enriched per gm. of ^{18}O, £225.

The ^{14}C radio-active isotope is made in the pile, whilst ^{13}C is present in all carbon to the extent of 1·1 per cent., ^{15}N in the atmosphere to the extent of 0·38 per cent. and ^{18}O in water to 0·2 per cent.

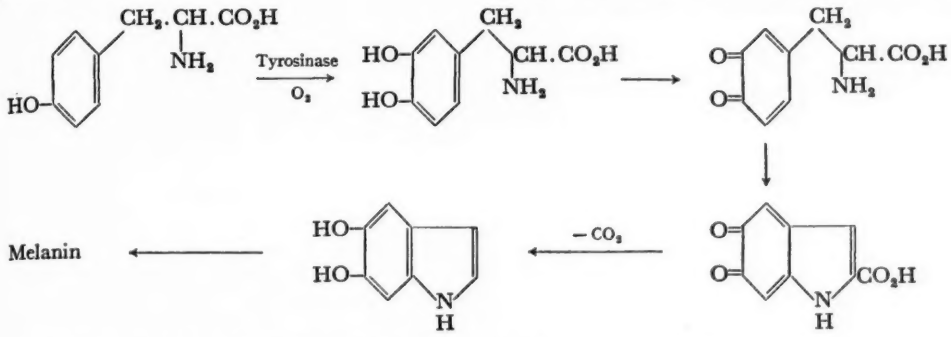
It is not surprising, therefore, that the bulk of the work done with them has so far come from the United States.

Nevertheless I want now to indicate how ^{14}C has been used to gain a further insight into the mechanism whereby melanin pigments are formed from tyrosine.

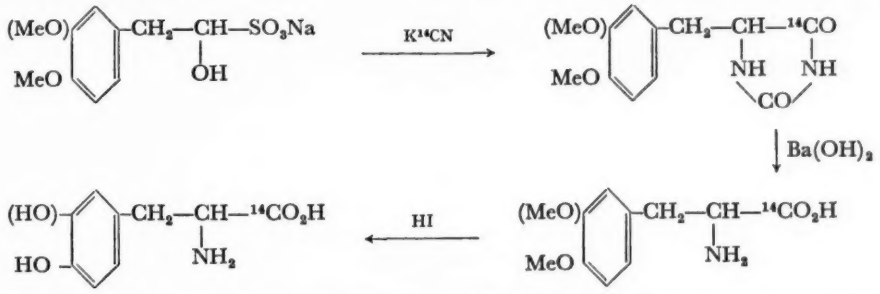
In the majority of organic reactions involving a rearrangement of the carbon skeleton (e.g. through the migration of a carbon atom) or the elimination of a carbon atom, it is fairly obvious which atom has migrated or has been eliminated. There are, however, a few reactions in which this is not so obvious; and in which classical methods could not give, with certainty, the answer. In such a case, the use of isotopic carbon (^{13}C or ^{14}C) as a tracer can provide the answer, provided (a) that it is possible to synthesise the compound concerned specifically labelled with isotopic carbon in the appropriate position or positions, and (b) that (i) in the case of rearrangement reactions, the rearranged isotopic compound can be degraded stepwise so that the position of the labelled atom may be determined, or (ii) in the case of a reaction involving the elimination of a carbon atom, the resulting one-carbon compound can be isolated.

An example of the latter type is in a recent study of the formation of melanin from tyrosine, 3 : 4-dihydroxyphenylalanine, 3 : 4-dihydroxyphenylethylamine and 5 : 6-dihydroxyindole. So far, the most important contribution to our understanding of the structure of melanin and

of the reactions involved in its formation is due to H. S. Raper and his collaborators, who suggested the following scheme for tyrosine :—



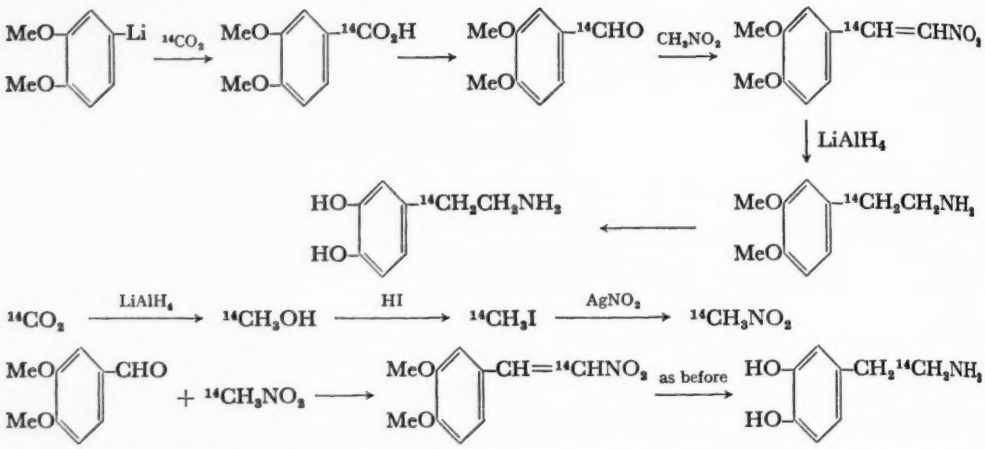
According to this scheme, the last stage of which has been established by the successful synthesis of 5 : 6-dihydroxyindole by Robertson, the carbon dioxide which arises during the formation of melanin from tyrosine has its origin in the carboxyl group of the amino acid. Tyrosine and 3 : 4-dihydroxyphenylalanine labelled with ^{14}C in the carboxyl group were therefore prepared as follows :—



When the resulting amino acids were treated with oxygen at *p*H 8 (in the case of tyrosine, also in the presence of tyrosinase), the evolved carbon dioxide, although radioactive, had only about half of the activity expected if it originated entirely from the carboxyl groups. It therefore follows that part of the evolved carbon dioxide originated from one or more of the other eight carbon atoms of the amino acid molecule. Moreover, it was also found that carbon dioxide is evolved when melanin is formed from the carboxyl-free compounds 3 : 4-dihydroxyphenylethylamine and 5 : 6-dihydroxyindole by passing oxygen through these solutions at *p*H 8. That this carbon dioxide should result by the complete breakdown of the molecule under such mild conditions is unlikely, and, moreover, it has been shown that the carbon dioxide evolved from 3 : 4-dihydroxyphenylethylamine labelled with ^{14}C in either the α or β position of the side chain is of very low activity. These compounds were synthesised as shown on page 130.

It therefore seems fairly certain that the carbon dioxide originates mainly from the benzene ring. We have shown that catechol also evolves CO_2 under oxidative conditions and this can only come from the opening of the ring.

With regard to *the* great problem of cancer, apart from the limited, but valuable treatment possible by therapeutic agents, and the use of radiation therapy, the real advances in treatment are still ahead of us and may yet baffle our researchers for many years. It would seem, therefore, the merest common sense to strain every effort at prevention, particularly since some steps to this end are easily practicable. Statistics have been published which show that cancer in the respiratory system is on the increase, and this naturally focuses attention on the air, or rather smoke, we breathe, whether

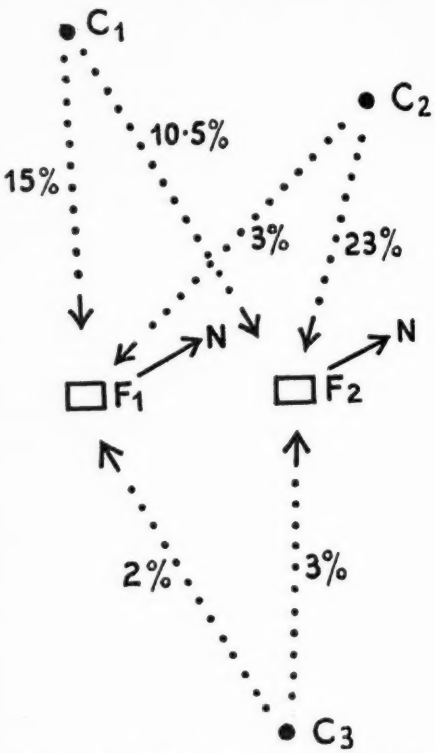


deliberately or unavoidably. The Registrar General's statistical review for 1948–1949 states that the crude death rate for cancer showed a continuing steady rise and much of the male increase was attributed to cancer of the lung. Thus the death rate of 866 per million living at ages between 55–65 in 1940–44 had reached 1,520 in 1948 and 1,637 in 1949.

It was a surprise to learn recently that no single organic constituent of the smoke scourge has been isolated and definitely identified, although for too many years past this evil has been stressed in and out of season.

The smoke on which I wish to make a preliminary statement has been collected in a successful effort to keep our new Chemistry Building at Newcastle clean, for although Newcastle is a clean city as cities go, it has some murder-emitting chimneys and thousands of buses there as elsewhere belch forth their black smoke, all of which could be prevented. As but one instance of what must be hundreds, along one ten-mile stretch of main road between Newcastle and Hexham there are no fewer than eight schools and hundreds of buses and lorries emit black smoke along this route all the day long. It is impossible to state the extent of the damage to health thus incurred and probably only revealed after the lapse of years and all of it preventable.

The smoke which I have been examining comprises that commonly present in large centres of population and industry, but its composition will also be affected by the proximity of three chimney stacks to the filters, two serving heating installations and fired with small coal, and the



	Distance in Yards
F_1–F_2	40
C_1–F_1	50
C_1–F_2	60
C_2–F_1	70
C_2–F_2	65
C_3–F_1	60
C_3–F_2	50

third an incinerator for the disposal of surgical and other refuse from a large hospital. The diagram on page 130 shows the relative positions of the filters and the wind directions for the six months involved.

The filters are worked by two fans which together discharge 44,000 cu. ft. of air per min. on summer duty and 29,000 cu. ft. in the winter, and the filter is made up of 54 sections of cotton wool on a gauze background, a quarter being changed every three months approximately, and the whole therefore renewed yearly.

An account of the results obtained will be given at this point at the meeting.

In conclusion, many other great problems await solution, by the chemist, such as the full harnessing to our rightful needs of the vast amount of vegetation produced in tropical regions, and on which a start has been made by the Colonial Products Research Association.

The economic implications of such work will be self evident and will recall to your minds the effect of the commercial production of indigo in Germany on India and of alizarin on France. Further, the impact of the present-day large-scale replacement of soap by synthetic detergents on the raw materials produced in West Africa and elsewhere will make more than our river boards foam at the mouth !

In spite of all his achievements the chemist is still from time to time said to be narrow minded. If the criticism were that he is special minded I for one would not object. After all, this is the atomic age and the mind of the present-day chemist necessarily differs from what it was in the bow-and-arrow days.

My thanks are due to Miss N. S. Brown, B.Sc. for help with the literature survey, and to Dr. G. A. Swan and Dr. F. K. Duxbury for the work on ^{14}C.

FOSSILS AND THE EVOLUTIONARY PROCESS

Address by

PROF. T. NEVILLE GEORGE

PRESIDENT OF SECTION C

TIME AND CHANGE

FOSSILS, for geologists, are the stuff of evolution : they set evolutionary process in a perspective of time, and they provide substance for evolutionary theory that is only to be inferred by neontologists living (on the geological scale) in a momentary present.

The main stages of advancing palaeontological knowledge are clear. The graded structural series, collateral witness to the transformation of species over periods of geological time, was an expository model that made its appearance within a few years of the publication of *The Origin of Species*. Its illustration in Huxley's equids, Waagen's ammonites, and Hilgendorf's planorbids stamped a mark of method that was not to fade for many decades. Informed by Bather's seriation and Swinnerton's bioseries, it continues to be an indispensable analytical tool in evolutionary morphology.

But the morphogenetic line of structures is static, outside time, a fictive line of artificially segregated organs and biocharacters, not a line of once-living organisms. It has manifest shortcomings as a model, and before the end of the nineteenth century its deficiencies were recognised, and it was refined into the phyletic lineage of chronologically graded forms and applied to actual fossil series. The classical instance continues to be Rowe's study of the Chalk sea-urchins.

The lineage was given body by Carruthers, whose zaphrentoid corals showed that a view of an evolutionary series as a succession of individual transients strung like knots along a single genealogical thread was altogether too naive and abstract.

The implications of his work were made fully explicit by Swinnerton and Trueman in their pregnant concept of the lineage as a plexus of anastomosing threads linking successive generations. Exemplified in Trueman's analysis of the evolution of Liassic oysters, this fulfilment of sixty years of palaeontological theory initiated and anticipated lines of advance leading directly to the present-day synthesis of the stages and the modes of evolution. The phyletic series is now seen to be composed of successive communities or populations, each an association of variant individuals in intricate plexiform morphogenetic relationship with the members of ancestral and descendant populations, and each conveniently defined in statistical terms by reference to variation-range and the modal expression of appropriately measurable biocharacters. Evolution then appears as a series less of structural changes than of graded chronoclines in population-cluster.

The analogies with the findings of genetics are close and obvious, and the extrapolation of genetical theory to the mechanism of evolution leads to conclusions that accord closely with palaeontological theory.

Thus amongst permanently parthenogenetic forms (if any exist) the linear thread of heredity may be a strictly apposite metaphor, for the one-to-one correspondence of ancestor and descendant is maintained along its length ; and its fossil equivalent is the (as yet imaginary) line of evolution linking specimen with specimen, staggered by periodic mutation.

But when reproduction is biparental and dioecious, as it is in the great majority of animals, conditions of more or less free panmixis operating in a richly composite

demic gene-pool bring about indiscriminate cross-breeding amongst parents and an ensuing wide range of genotypic (and thus phenotypic) variants in the offspring. As an individual each variant then displays structural departures from the norm of its generation, for it reflects a ' random ' aggregation of genes from the available gene-pool ; and it is merely an incidental unit in a host of other variant units that may by chance survive to participate in producing the next generation of the evolutionary population-sequence.

The palaeontologist provides in his developmental series of forms (filled out in the lineage into series of populations), as in the modal grades of Trueman's oyster-series, the evidence of continuously progressive shifts in population-cluster. Between the members of successive populations he observes plexiform morphogenetic relationships. The geneticist interprets the shifts by reference to sustained panmictic gene-shufflings repeatedly modified by 'random' mutations and expressed in genotypic variants, and to a changing genic balance in successive populations arising through the differential encouragement or suppression of different kinds of phenotypic variants by environmental ('natural') selection. Between the members of successive populations he observes plexiform genetic relationships.

Such theory incorporates the evidence of many fossil series (only to a degree less obvious in invertebrates than in vertebrates) that evolution is almost always adaptive. In each generation a proportion of individuals, viable in the environment in which they find themselves, survive long enough to reach reproductive maturity. Descendant individuals, by adjustment of function (and correlated structure) in conditions of changing environment, continue in successive generations to reach maturity in sufficient numbers to ensure that group-survival is maintained. But the theory confers upon adaptation a statistical meaning far removed from a crudely assessed correspondence of specific function and specific structure, and from any finalist or purposive implications of inevitable or desired ends.

Adaptation may be looked upon as an equation the factors of which are on the one hand all the variant individuals collectively forming the lineage, and on the other the lineage's total environment. It is measured by the relative numbers of variant kinds surviving to reproductive maturity in successive generations; and the survivors are no more than members of a statistically preferred kind. Under differential selection, the adaptive process is displayed by the lineage in a passive and accommodating response to asymmetrical selection pressure through a proportionately high mortality of unfavoured variant kinds as shown by a modal shift in successive populations. A sustained modal shift along a particular ' adaptive ' trend merely reflects ortho-selection ; and pre-adaptations are accidents. To ask if a single individual is adapted to its environment is thus meaningless : as a random isolate it is not part of the statistical model. Any Lamarckian or finalist concern with the ' perfecting ' of structural instruments for particular uses is thus by-passed.

Evolutionary Divergence

The forking of lineages, the diversification of stocks of common ancestry—exceedingly common in nearly all groups of fossils and an essential feature of long-continued evolution—cannot therefore be glibly attributed to an unexplained ' adaptive radiation,' even though the phyletic branches in fact radiated from a common stock and did so by a (technically defined) process of adaptation. Explanation of the manner of divergence (Rensch's cladogenesis) must accord consistently with what is known or inferred of the evolution of successional populations and of the relations between contemporaneous populations.

The evidence provided by living organisms, marshalled chiefly by Mayr, strongly supports the view that conspecific populations, at least of most metazoans, become genetically divergent and tend to follow independent evolutionary paths, ultimately to become specifically distinct, only when they are more or less completely isolated from one another, and when cross-breeding gene-flow between them is consequently greatly reduced or ceases altogether. The isolating factors may be local and abrupt, often topographical barriers ; or they may be simply a function of distance, as in the case of species (like those of many marine

invertebrates) with extended geographical range in which are developed through differential environmental selection clines of prolonged gradient.

Attempts have been made, as by Newell and Sylvester-Bradley, to apply the general case to invertebrate fossils by constructing hypothetical conditions in which barriers are built and migration induced. But although the expository picture is instructive it is based solely on analogy, being an extrapolation in both time and situation from what is known of living forms. Even amongst living forms the main evidence is provided by land or inland-water animals, many of them vertebrates, on which the effects of clinal grading by distance or of clinal fragmentation by physical barriers are not difficult to make out.

Very little is directly known of the actual modes of evolutionary divergence in marine animals. Phenotypic clines are certainly of common occurrence especially in shallow-water forms, and have been recorded in corals, echinoderms, crustaceans, brachiopods, molluscs, and such ground fishes as the plaice and sole. This is not surprising since most of these organisms live in neritic environments providing great contrasts in local conditions. Embayments, headlands, soft or hard sea-bottoms, and a host of other physical and biotic factors encourage the emergence of microdemic contrasts. Some of the clines are without genetical significance, as, for instance, the brief cline of high- to low-coned limpet shells between high- and low-water mark. But even when they are partly of genetic origin, the wide dispersal of the usually planktonic eggs and larvae (and amongst the nekton of adults too) tends to break down any incipient segregation and to re-impose the need for differential selection in each generation.

The extended clines become fragmented and begin to display sustained genetical divergence by the permanent extinction of annectant populations only when the species-range greatly exceeds individual dispersal range (at any stage of life), and usually the remnant populations must exist in separated areas until reproductive isolation is established. Amongst pelagic and widely free-swimming organisms the opportunities for segregation are correspondingly diminished, and ' cosmopolitan ' species, so long as they remain cosmopolitan, are unlikely to give rise to divergent offshoots. A corollary is that the single lineage cannot fork into two or more independently evolving branches within a limited area, for continued panmixis would prohibit segregation.

This implies that in the free waters of the seas simple ' adaptation ' (to different foods, or swimming habits, or predators) needs to be qualified as an explanation of the divergent evolution of many groups of oceanic vertebrates. Thus sirenian evolution, the animals being herbivorous, falls into the general physical framework of pocketed environments, with fragmented range a powerful aid in diversification (the last living survivors being significantly restricted to different oceans or to opposed oceanic coasts). Similarly the river-dolphins, and many groups of fishes like the living dipnoans, may be regarded as segregated off-shoots of open-sea ancestors. But the various whales and porpoises and most of the dolphins are not so easily accounted for. Presumably in the past they must have been much less cosmopolitan than they are now, with limitations of group-range imposed possibly by distance, possibly by breeding habits, possibly at times by tectonic barriers (so that they evolved for periods in more or less self-contained basins). The plesiosaurs, in their divergence into small-headed and large-headed series, both apparently open-sea groups, present a similar problem ; and the fast-swimming ichthyosaurs intensify it. A general inference to be drawn from the relative difficulty of reproductive isolation being established in pelagic forms is that many adaptive radiations in fishes probably originated in shallow-water forms, possibly ground-dwellers.

The difficulties, on current theory, of evolutionary divergence in limited areas also make a great number of reconstructed invertebrate phylogenies suspect—as for instance those of Woods's inoceramids, Spath's liparoceratids, Lang's cheilostomes, Fenton's spirifers, Brinkmann's cosmoceratids, George's reticulariids—unless the limited area in which the fossils are now found was in fact during evolution expanded into a wider area of migrant range.

Under uniform marine conditions, even in shallow water, the occurrence of undifferentiated neighbouring populations is to be expected ; and it is as common amongst fossils as it is amongst living organisms. Thus Parkinson has demonstrated the virtual sameness of populations of schizophoriid brachiopods in the Craven reefs ; and his conclusions have been supported and elaborated by Wright, who has found that in the same ecological (lithological) setting populations of martiniids also display a statistical identity. It is probable, therefore, that these two respective groups lived, as might have been expected, too near each other to display significant differences, and thus give no sign of evolutionary divergence (and incidentally, in also giving no sign of successional change, they confirm the stratigraphical correlation of the rocks in which they are found).

At the same time, however, expectations of uniformity in neighbouring populations are not always fulfilled, and McKerrow has recently recorded the occurrence of statistically significant clines, precisely comparable with those of living shallow-water molluscs, in ornithellid brachipods of the Fuller's Earth Rock. This is the more surprising since the collections were made along a relatively short outcrop in a lithological matrix seemingly indicating complete uniformity of conditions (though such variable factors as food-supply and salinity, not recognisable in the present-day rock, may have had marked effects on growth, and the outcrop runs athwart the Mendip axis). The thinness and continuity of the Rock make it a fair assumption that the populations collected can be regarded as quasi-contemporaneous : certainly few fossil populations are likely to be more finely identified as of the 'same' age. Along such a short outcrop (some of the variant populations are only a mile or two apart), the degree of genetical divergence could not have been great, and perhaps the variation was mainly non-heritable : but nevertheless the cline itself implies differential selection and the possibility of change in genic balance.

Opportunities for such clines to become disjunct may be provided by even slight changes in local physical controls (including not only changes in food-supply and salinity but also changes in depth, temperature, aeration, current-flow, muddiness), the occurrence of which on a surprisingly severe scale in a mere two feet of apparently uniform rock has been demonstrated by Craig in his palaeoecological studies in Scottish Dinantian strata.

The lineage of the ornithellids is not known in beds younger than the Fuller's Earth Rock ; and it is the general case that the three variables of topoclinal gradient, physical determinants of isolation, and 'vertical' phyletic branching are never likely to be discovered completely in any single fossil series. But fragments of theory may be pieced together and applied in favourable cases, as to the Liassic oysters.

Swinnerton has shown the species-groups of *Gryphaea incurva* and *Gryphaea cymbium* to be sympatric and to have diverged from a common ancestral stock in early Liassic times. Their physical environment (an open though probably shallow sea) would seem to have precluded any likelihood of spatial discontinuities in the British area. Nevertheless, the lineages, despite their abundant representation at many horizons, are broken repeatedly by faunal diastems, and in fact their existence as lineages is on present knowledge a reconstruction. If clinal grading like that shown by the ornithellids may be assumed for the Liassic oysters (a necessary assumption if Swinnerton's phylogeny is accepted), the diastems may signify occasions when disjunction occurred.

Disjunct clines maintained for even a short period of geological time may result in permanent reproductive isolation (speciation), even if the clinal remnants should subsequently return to the common ancestral ground. Moreover, changing physical conditions leading to disjunction also lead to migration. The diastems interrupting the graded oyster-series show that for long periods the lineages were almost, perhaps completely, unrepresented in the British area. It is unlikely that during these migratory episodes the *incurva* group and the *cymbium* group remained travelling companions the whole time, if there was (as there must have been) initial clinal disjunction. The period of

evolution of the main *incurva* group spanned perhaps a million years (the order of Straw's figures may well be accepted), of the *cymbium* group two million—ample time for reproductive isolation to be established, even if divergence was initiated as late as the end of Hettangian times.

Much of the argument in this example of divergence is speculative. But the oysters provide one of the most completely preserved records of fossil lineages ; and the very fact that the intergrading populations of most other inferred lineages are fragmentarily represented in any one area is itself a pointer to repeated migration. In its turn migration implies discriminatory environments, sometimes directly recognisable in the rocks as crude facies contrasts. The general thesis of geographical speciation may thus be reasonably applied to fossils, even if they do not furnish proof of it, and the process of sympatric speciation excluded from the evolutionary mode of marine invertebrates.

Categories of the Evolutionary Mode

Although evolution is divergent, so that different species of common ancestry may be contemporary, in evolution all species in origin are successional. In this apparent antithesis lies much of the disagreement on methods of systematising phylogenetic process. On the one hand Burma has declared that a species is ' a fiction, a mental construct ' that tends to obscure true relationships. On the other hand Mayr has insisted that a species (normally) is there to be observed as a clearly defined entity, a natural population, a category of individuals each bearing an exclusive genetic-functional relationship to its fellows as definite as that of a municipal voter or an apprenticed tradesman to his. The disagreement is no doubt largely verbal : the protagonists lay emphasis on different aspects of evolution, Burma on evolution as though it were wholly linear-successional, Mayr as though it were wholly timelessly divergent.

Everyone, Burma included, would agree that Mayr on his specified genetical criteria (at least in their application to metazoans) is right in asserting that only in exceptional cases is there any actual difficulty in recognising the different species in a given area at any one time—the layman indeed often (and the species-member always) making the recognition long before the professional taxonomist arrives on the scene.

Moreover, genetical species are not contemporary accidents, non-recurrent freaks, for they simply are the products of evolutionary divergence ; and at any moment in past geological time it may be presumed that such species, as the products of former divergence, were no less clear-cut ' objective ' genetically separated groups than they are now. In fact, if evolution (of metazoans) is in fundamental origin monophyletic or at most oligophyletic, then virtually all species at every moment of past time were of Mayr's kind.

Two major defects, however, make Mayr's systematics unacceptable in practice to geologists : his genetical criteria are not applicable to fossils, and his species are as categories (but not in origin) functionally determined and non-developmental.

The equating of morphological with genetic criteria is a common (though usually tacit) means of overcoming the first difficulty : in the long practice of phylogenetic (as of other) classification it is customary to use selected characters as guides to relationship in establishing lineages and taxonomic groups. And while there are dangers in the use (conscious or not) of morphological norms as standards—dangers that have notoriously confused systematics in the past—it remains true that even amongst the most obviously ' good ' fossil species, like *Goniorhynchia boueti* and *Carbonicola protea*, of which hundreds of specimens may be found as members of a ' non-dimensional ' population-sample in the same bed within a few feet of each other, the final criterion of specific membership (that is, of belonging to a unitary population or population-group) lies in comparative morphology : the outsider finds no place in the biometrical sample.

The morphological criterion is nevertheless rarely as precise as the genetical. Thus amongst the athyrid brachiopods of the Dinantian rocks of the South-Western Province, the *composita* group of the *Seminula* zone includes variants that are

ambigua-like in the development of plications ; but statistically they fall within the variation-range of *composita* and may be regarded as belonging to that species. But similar athyrids of the Tournaisian rocks, elongate like *roissyi* or broad like *glabristria*, are found in communities that display bimodal variation curves : *roissyi* then is morphologically a distinct species from *glabristria*, but a considerable number of specimens are nondescript and are not readily placed in either.

The morphological criterion is, however, usually in practice less oblique than that afforded by accommodation in a population-sample : it is that of comparison of specimen with specimen. This may be on an ' objective ' measurable basis (though the measurements are not always made). Individuals having the ' same ' or closely accordant characters are then grouped into non-arbitrary but artificial ' morphological species.' The approved measurable interval between species must nevertheless always be a subjective or a conventional one, and has none of and almost inevitably must depart widely from the ' natural ' objectivity of a genetical barrier ; and it makes no provision for the great number of individuals displaying independently variable biocharacters. Since the ' trends of variation ' in any population-sample (trends subjectively recognised, useful in exposition but having no necessary relation to trends of evolution), as illustrated for instance in Leitch's pioneer work on the anthracosiids or in Aitken and McKerrow's work on *Goniorhynchia boueti*, are permutatively combined in the actual specimens forming the sample, the ' morphological species ' at best includes forms closely similar in only some selected characters.

The morphological norm for comparison usually becomes the morphotype. This may be abstract, idealised, an echo of the formal pre-Darwinian archetype, to which individuals, live and variant, approximate in more or less degree ; and it may be regarded, as by Schindewolf, as having some real foundation if there are macro-evolutionary mutational steps between species. But in most cases the norm exists as the sample individual (like the holotype of official taxonomic nomenclature).

The elevation of an exemplary type, conceptual or individual, of a species to a categorical standard of comparison does violence, however, to current genetical-palaeontological theory, no matter how necessary it may be for nomenclatural consistency. The individual as such does not evolve (except perhaps in ontogenetic development), and most individuals, including it may be guessed many holotypes, have no place except as end-products in evolution—for they die, the vast majority before maturity, without leaving descendants. The evolutionary unit is the population : the lineage is composed of successive populations : and evolution is expressed in the systematic shift of population variation-range and variation-mode.

As a member of its population and population-lineage each individual has equal status with, and is no more and no less typical than, every other individual member. As a genetically permutative phenotypically random variant, it always departs in more or less degree (more in some characters, less in others) from the statistical norm of its population—and incidentally finds a complementary place on each ' trend-line ' of variation. Impressive morphological contrasts between individual intraspecific variants thus afford no ground for the special designation of morphotypes in evolutionary studies (however useful labels may be in stratigraphy) : a point well illustrated by McKerrow, who has shown that the statistically defined species *Ornithella bathonica* incorporates variants that formerly were allocated to five morphotypic ' species.' Morphotypes are acceptable pegs upon which to hang species only when their population-samples are very small, and when they represent in the existing state of knowledge rare kinds sufficiently different in structure from other morphotypes or populations to make the possibility of conspecific relationship remote.

The second defect for palaeontology in Mayr's genetically defined species—that they are categories of the moment outside the dimension of time—is sometimes reflected in the statement, metaphorically appropriate, that the palaeontological equivalent of a neontological species is a cross-section through a lineage. The

accidents of lineage-preservation normally involve large discontinuities in the record of any one limited area that result in the natural provision of widely spaced cross-sections ; and if the bed in which the fossils of the cross-section are found is sufficiently thin the species represented may be effectively equivalent to a genetical ' non-dimensional ' species. Fossil species are then, on the time-plane of the cross-section, almost as easily delimited, on derived ' objective ' and ' natural ' grounds, as living.

But such accidents of preservation, though highly convenient in taxonomic labelling, impose a criterion of ignorance on systematics ; and with increasing knowledge of stratigraphy and fossils their effects tend to diminish and ultimately to vanish. When lineages come to be reasonably well known, independent treatment of disparate populations ceases to be possible. Natural cross-sections are still offered for examination in successive stratal layers, but contiguous cross-sections are in bulk so little different from one another that no one would contend, merely because they are not exactly of the same age, that they belong to different species. The systematised description of the relationships of the actual members of the graded population-series cannot overlook the continuously transitional sequence of forms or neglect the time-dimension.

The essential problem has long been recognised. It is that of dividing up a continuum into manipulable segments ; and as it now affects palaeontology it is not entirely without some echo of the paradoxes of Zeno the Eleatic. The past history of taxonomics has imposed a static Linnaean classificatory system on dynamic evolutionary series ; and the discrete units of that system accommodate the transient forms of intergrading communities no more easily than a conceptual fragmentation of time into moments and space into loci accommodates the fact that Achilles could catch the tortoise.

Given the graded series sufficiently well documented by fossils, there are various devices that may be (and in part have been) adopted to designate the momentary and the serial stages of change. The description of the single population (the local cross-section of a lineage) has been elaborated over half a century, in recent years notably by Leitch and Eagar, to a degree of statistical refinement that leaves no room for doubt or personal bias in its evaluation ; and the methods adopted, mainly of assessing variant-character distribution by graphical and algebraical means, may be extended to cover whatever biometrical details may be looked upon as significant. The place of ' types ' in such a biological context is wholly factitious.

The precise expression of evolutionary change in successive populations is more difficult, partly because the duration of geological time on the scale of the changes is not known in absolute terms (though as a first approximation it may be equated with thickness of deposit), partly because rates of evolutionary change are not constant and therefore are not a simple function of time : they vary between lineages (allowing evolutionary divergence to occur), they vary between one bio-series and another within the lineage, and they vary (for any particular biocharacter) between one time-interval and another within the lineage : and in their manner of variation they frequently suggest the effects of allometric gene-controls. Nevertheless, superposed variation-parameters on a conventional graphical frame have already been effectively used to illustrate population changes, collective ontogenetic changes, and population contrasts, and are incomparably more informative than a speculative statement that (Linnaean) Species B has evolved from Species A. But no formal attempt has yet been made to integrate the regression formulae of successive populations.

Names and Things

It is a great convenience to have a name for a thing, even when mathematical formulae can say much more about it ; and the species of the timeless present may be called with comprehensive and exact meaning *leo* or *tigris* or *pardalis*. Comparable species living in moments of the past, each recorded in a single band of fossils, may similarly be called *boueti* or *mulleti* or *aegiranum*. Analogous (but not homologous) names may no less conveniently be given to population-series, segments of lineages ; and the appropriate plexus of Liassic oysters be called

irregularis or *incurva* or *cymbium*. But the one kind of name is different from the other, and confusion between the two arising from their homoeomorphic similarity—the sort of confusion that has been general in the past—can only lead to confusion of understanding of evolutionary processes and of the meaning of species—and confusion of the kinds of things named.

When Mayr insists on the actuality of existent living species in nature he is in direct descent from Linnaeus, who would have appreciated his criteria; and his species are in kind Linnaean species. When Burma, epitomising a consensus of much palaeontological opinion (if not of explicit doctrine), contends that in the evolving lineage the species is not given in nature but is a fiction, he is asserting that Linnaean species, clear-cut and self-contained, have no useful or meaningful place in the time-sequence of transients constituting the lineage: applying Linneaean nomenclature to parts of evolutionary series is then like calling an hour an inch because they both happen to be measures of intervals.

In the unbroken gene-flow of evolutionary transition the lineage is the only natural unit; and, carrying logic to an extreme, some palaeontologists have argued that the species as a real entity, if it is to be recognised at all in evolving forms, should be equated with the lineage. Although this goes too far to serve practical ends, it rightly drives home the point that the categories of a species-concept applied to evolutionary series must (in contrast to Linnaean species) have extension in time, and must therefore incorporate a time-factor in their definition.

The classificatory fragmentation of time into conventional units is of course a general practice—into hours and years, into dynasties, into geological periods. The principle of applying species-names to segments of a lineage is therefore not intrinsically incongruous; and to a degree the practice has over many years made an oblique entry into stratigraphy in the designation of zones (notably those, like the *bifidus*, *modiolaris*, *semicostatum* zones, not wholly assemblage zones). The segments of a lineage, however, are continuous with those going before and those following after, and in classification must therefore be conventionally bounded (with correlative stratal limits at specified horizons); and if the members of a chosen segment are to be called *constricta* or *obliquata* the limits of the segment (and so the field of the members) must be chronologically and not morphologically determined.

The species *constricta* might thus be conventionally defined as including all those zaphrentoid corals belonging to the *delanouei* lineage of the *Dibunophyllum* zone; and the species *obliquata* as covering the segment of the *incurva* lineage of the lower *bucklandi* zone.

Since, however, geological time-pieces refined to give absolute intervals on the scale of the known evolutionary series have not yet been constructed, and the timing of zones itself ultimately rests on a palaeontological foundation, the evolving organisms can themselves be used as substitute clocks. And an alternative and much more convenient nomenclatural device is one in which the chronological species covers a defined range of evolutionary progress. Given the expository frame of evolution as one of intergrading populations amenable to morphological bulk-description by means of variation-graphs, shifting modal peaks, and regression formulae, the factual data of a lineage-segment may then be described, presented, and labelled as in the hypothetical model on page 140.

Such a scheme, carried by multi-variate analysis to any desired depth of detail, leaves in no doubt the substantial content of the lineage in both time and space and the classificatory groups designated by the specific names. Nor need the specific names be Linnaean in form (except by statutory fiat). The binomial strait-jacket is one into which only a crippled description of a lineage could be made to fit; and a mathematical or numerical index or catalogue formula would be much more appropriate, accurate, and informative than Greco-Latin nouns and adjectives. A species so described would then approximate closely to Simpson's hypodigm.

It may be objected that the scheme makes no provision for the individual variant. But the individual variant is unique, and as such is not amenable to

	Variant-character parameters	Modes of variation-range and standard deviations		Regression formulae	Nomenclature
		m/n	*p/q*		
Zone F . .	UVW	$k_6 : \sigma_6$	$t_6 : \sigma_{12}$	$a_6 = b_6 c_6^{x_6}$	Species Z
Zone E . .	RST	$k_5 : \sigma_5$	$t_5 : \sigma_{11}$	$a_5 = b_5 c_5^{x_5}$	
Zone D . .	PQR	$k_4 : \sigma_4$	$t_4 : \sigma_{10}$	$a_4 = b_4 c_4^{x_4}$	Species Y
Zone C . .	MNO	$k_3 : \sigma_3$	$t_3 : \sigma_9$	$a_3 = b_3 c_3^{x_3}$	
Zone B . .	KLM	$k_2 : \sigma_2$	$t_2 : \sigma_8$	$a_2 = b_2 c_2^{x_2}$	Species X
Zone A . .	GHJ	$k_1 : \sigma_1$	$t_1 : \sigma_7$	$a_1 = b_1 c_1^{x_1}$	

classification but only to description. Even as a variety (equated with its ' morphological species,' its class of individuals, including parallel mutants, phenocopies and real sibs, selected from any populations of any age, having a general similarity of form) it is relatively rare and is artificially or accidentally abstracted from its biological matrix. The scheme is a classification of populations into which the individual is absorbed. It is now a commonplace that as an isolate the individual is often specifically anonymous because variation-ranges of successive populations may spread their mantles over it from one side or another, and then it ceases to be identifiable precisely because it is not seen in its own population-context. This is but the classificatory reflection of the fact that the place of the individual in the formularising of the evolutionary process is always incidental.

The species of the lineage-segment then covers not a range of morphological types but a range of population-modes. Its interval may be conventionally standardised, despite the elastic stretch of time spanned, by defining the mean change between species. This in effect is embodied in the criteria suggested by Burma and Haldane when they make the range of the ' chronological species ' a function of the standard deviation in variation-spread. A somewhat similar proposal, in which, however, the full range of variation and not the form of variation-scatter (kurtosis) provides the unit interval of quantitative change, has been made by Simpson to suggest a conventional basis of distinction between species and subspecies : statistically significant differences between populations having overlapping variation-ranges are at the subspecific level, those having no overlap of variation-range at the specific. (The apparent ease with which the individual variant may then be referred without hesitation to its species is illusory, as Simpson himself has pointed out, for his conventional definition makes no provision for intermediate populations. Since the development of mutually exclusive variation-ranges between successive species is accomplished only over relatively long intervals of time, intermediate populations with variation-ranges partly overlapping those of both the older and the younger statistically defined types must, with all their variants, arbitrarily be placed with one or other. The alternative, to refer the individual variant of the intermediate population to the species within whose variation-range it falls, leads to the classificatory fragmentation of the population-unit that is being classified.)

The Range of Variation

Phenotypic contrasts in any group of organisms are more extensive then genotypic, and the actual range of variants in a given population at any moment is but a part of the potential range of variants that might grow and survive under less rigorously selective natural conditions. In short, variability is normally wider—much wider—than the variation displayed. Thus in the *Zaphrentis* zone in South Wales modal contrasts between neighbouring athyrid populations suggest the changing influence of currents and aeration (and doubtless other factors) on growth-form

and variation-range : specimens found in crinoidal limestone are relatively small and elongate, while only a few hundred yards away specimens in oolite virtually but not exactly of the same geological age are large and broad. Similarly Trueman long ago showed that contrasts in widely spread contemporaneous populations of Carboniferous anthracosiids occur in many coalfields and are to be attributed to environmental controls.

The relevance of the relation of variability and variation to evolution, taxonomics, and stratigraphy has been most clearly brought out by Eagar, who in his studies of recent and fossil fresh-water mussels has shown that wide variations of shell-shape in living unionids controlled by ecological station are paralleled by similar variations in the anthracosiids controlled by lithological station. His work, notably in his demonstration of the correlation of silting and carbon-content with size and growth-shape of members of the Namurian *exporrecta* group, offers closely analysed instances of the general effects of the influence of facies on fossil form (a relic of the original complex facies - aspect being preserved in the lithology). It is obviously important in its pointing to the fossil occurrence of phenotypic population-modes (ecophenotypes) that may not be entirely or even mainly genetically controlled—modes that can fluctuate with fluctuating external controls.

The gradations of clines show that the broad areal replacement of phenotypic forms in living organisms is mainly effected by genotypic change. The stepped or broken cline, usually the product of physical disruption and maintained by physical barriers, provides the ideal conditions for the genetic differentiation of populations and the emergence of geographical subspecies. Like the lineage-species, the subspecies, which as a concept owes its origin to a study of the areal distribution of contemporaneous living animals, has no clear-cut existence, and thus differs from the precisely defined genetical species. By the accident of geographical isolation neighbouring subspecies may display sharp genetical (and phenotypical) contrasts ; but where there is speciation by distance the merging of subspecies may be complete. In the former case the spatial gaps are like the frequent temporal gaps in fossil series, and are equally a convenient but merely negative aid in taxonomy.

While no doubt the subspecies is incipiently a full species, due (if the process of divergence continues) to become a complete genetical isolate, it is as a subspecies conventionally and not naturally defined ; and its intrinsic qualities, apart from its geographical location, are morphological and biometrical and are assessed by studies in population statistics. Such intraspecific grades on the single time-plane thus offer close morphological comparison with the grades of a fossil lineage ; and it has been frequently pointed out that there are rarely any grounds for distinguishing between topoclines and chronoclines amongst fossils. Newell has consequently argued strongly for the recognition of the subspecies as a stage in the evolutionary transformation of species.

His recognition of ' an infraspecific category to denote the minute stages in evolution admittedly of less than specific rank ' may then be formally made ; but there remains the difficulty of evaluating stages ' admittedly of less than specific rank,' since the genetical criterion, arbiter between subspecies and species in living organisms, is not applicable to fossils. In palaeontology the subspecies must be a conventional subdivision of a species.

A relative chronology of fossils being given by stratal sequence, Sylvester-Bradley has suggested that the smallest statistically significant difference between successive populations, since it may prove to be of stratigraphical value, should be the criterion of a subspecies. As a working basis this might be acceptable if it did not have the obvious shortcoming that, as McKerrow has pointed out, the smallest significant difference between two chronodemes is dependent not on real morphological differences (though they must be there) but on the size of the samples tested. If the rate of evolution in the lineage is at no time reduced to zero, any two sufficiently large samples however closely successive might be expected to reveal significant differences.

Moreover, McKerrow's Bathonian ornithellids provide an instance amongst

fossils of what are effectively true topodemes, and show the dangers of imposing a stratigraphical measure on biological taxonomy. They also make a two-edged sword of Newell's arguments for the recognition of palaeontological subspecies, in so far as that category rests on the close morphological analogies that may be made between geographical and chronological subspeciation.

Statistically significant differences between populations involve both the range and the mode of variation. Thus the oysters of the *incurva* group at Fretherne are virtually identical with those of Loch Aline in so far as their relative breadth and curvature are concerned, but display significant differences in their relative height and the length of the area of attachment. By morphological convention therefore they may be looked upon as true subspecies (whether they are of exactly the same age or not). Swinnerton has suggested that genetical controls played some part in bringing about such phenotypic contrasts. On the other hand, McLennan and Trueman have presented a strong case for ecophenotypic adaptation in a highly ' plastic ' oyster stock that, as shown repeatedly in both living and fossil forms, readily makes ontogenetic adaptation to local environmental circumstances. It is notable that while the mode of character-variation between the two population-samples is markedly different, the range of variants is much the same in both. The general store of phenotypic kinds between which local selection could discriminate was thus common to the two areas ; and with an indefinitely large number of individuals available in each generation for selection, the preferred mode in all probability reflects in each area only an environmental bias. Even if some measure of genetical selection is superadded (differences in proportionate height or attachment-area implying different genic controls), the modal contrasts point to no more than fluctuations in genic balance and not to radical mutation : that is, the genotypic kinds available for selection were, no less than the phenotypic, common to the two areas.

Evolution in fossils is thus not necessarily proved by modal shift of population-cluster. It is even conceivable that subspeciation in geographically isolated areas might result not merely in marked modal contrasts between contemporary populations, but complete elimination of variational overlap. Thus close selection of ' high ' gryphaeas at Loch Aline and ' low ' at Fretherne would have restricted variation-range in opposite senses to the point of making the two populations appear to belong to different species, without there being any very great genetical differences between them : as Johannsen showed long ago, the extremes of pure-line bean populations may display 60 to 70 per cent. spread of phenotypic variants.

There is no doubt that, like all sustained evolutionary trends, the gryphaeoid trend in the evolving Jurassic oysters was brought about by genotypic changes, notably in the appearance of mutants displaying modified genic controls on curvature (spiral angle), on shell thickness, and on size : some of the genes may have been pleiotropic, since there is a suggestion of allometric bonds between the bioseries. Nevertheless, as Trueman originally showed and Swinnerton later elaborated, the variation-range of successive populations is wide, and in evolution on the whole gets wider. In consequence, many individual variants are notoriously not readily named or readily dated, and *dumortieri*-like forms may be found at all evolutionary grades from *irregularis* populations of the lower *angulatum* zone to *incurva* populations of the middle *bucklandi*. If Simpson's conventional definition of species-range be accepted, the differences between *irregularis* and *incurva* remain at the infraspecific level ; if Haldane's, they are intraspecific (but those between *irregularis* and *dumortieri*, *dumortieri* and *obliquata*, *obliquata* and *incurva* are not).

Moreover, although relatively incurved forms were increasingly favoured by selection as evolution advanced, it is clear that there was wide tolerance of differently curved variants at any one time. Selection pressure on the incurved forms seems to have been markedly asymmetrical in the later communities ; the favoured mode lay near the limit of tolerated curvature, but relatively flat forms, with large area of attachment, remained fairly common. While the most closely incurved forms may

no doubt be looked upon as the last stages of adjustment to life on a muddy sea-floor, the continued presence of the *dumortieri*-like forms in the latest populations clearly discounts any hint of compulsive drive or even intensely restrictive selection in the evolutionary process.

Further, the presence of a mixed association of variants in the latest populations obviously permitted the possibility of a reversal of the selective drive. For instance migrant forms might have found themselves in a hard-floor environment in which larger areas of attachment were appropriately adaptive. The *incurva* populations might then have become ancestral to successors with *dumortieri*-like norms. In such circumstances bulk-evolution would appear to be retrogressive, and the time-sequence opposed to the biological sequence. Some instances of proterogenesis might be explicable in such terms.

The moral of the oysters applies not only to phylogenesis but also to evolutionary description. If *incurva* displays no more than a subspecific difference from *irregularis* (since they have overlapping ranges of variants), or at best is but a single species-jump ahead, the categorical meaning of the genus is gone when the one is called *Gryphaea* and the other *Ostrea*. What usefulness the 'generic' name has rests in the trend it signifies, and recognition of this fact is implicit in Lang's fruitful concept, and Arkell's endorsement, of the genomorph.

Being a conceptual integration not of things but of categories (species), the orthodox genus as a category is wholly subjective. Even amongst living animals it is not a 'natural' category like a genetical species, and it cannot be recognised by intrinsic morphological or functional criteria. Amongst fossils, the lineage-species, if it is to be included in an appropriate genus, demands that the genus should also have a phyletic basis, and should be used as a name or other symbol to cover a range of lineage-species. Its measure must be arbitrary or conventionalised; but an extreme 'vertical' classification in which the genus contains only unforked lineage-segments (so that at each fork the generic name changes) is impracticable partly because lineages are known only fragmentarily, partly because divergent phylogenies are not easy to prove, and partly (perhaps chiefly) because species could never then be both congeneric and contemporaneous. Moreover, just as the place of the individual is uncertain when its population-species is unknown, so the place of the species may be equally uncertain when lineages are known only in fragments; and the reference of any one species to more than one genus is occasionally to be expected.

Ancestor and Descendant

The current interpretation of the evolutionary process tends to make the interbreeding population central. But the population, though a functional unity, has no existence apart from the individuals composing it. They are the guinea-pigs on which selection experiments, and they carry in their growth and form the product of phyletic selection and the imprint of their phyletic allegiance.

When succession in lineages is satisfactorily established, the mode of evolution (a mode repeated in oysters, unionids, fusoid gastropods, spiriferid brachiopods, goniatites, liparoceratid ammonites, holectypoid echinoids, and many other groups) throws light on ontogenetic growth, for it is (in the skeleton) very often effected wholly by changing rates of character-expression in successive populations—changing rates that result in shape-transformations. The extent of the changes implies continued mutation, and therefore, as Swinnerton has argued, the influence of repeatedly mutant rate-genes. In most cases the abrupt effect of mutant change is overlaid by the spread of phenotypic variation (and no doubt too by genotypic damping), and evolution appears to be continuously transitional; but occasionally, as in the divaricate ornament of nuculids or the ribbing of hoplitid spat, mutation may be a cause of visible discontinuity.

Since genic mutation (phenotypically dominant or not) usually occurs in the germ-cells of the mutant form, ontogeny as a resultant of genotypic nature and environmental nurture may be modified at any stage from the zygote onwards by novel genic controls. It may therefore reflect the pattern of ancestral ontogenies in all degrees from a close parallelism (as

in the gryphaeoid shell and the zaphrentoid cone) to wide deviation (as in hoplitid ammonites).

As elements of shape-transformation, however, the growth-rates of one biocharacter change in no absolute isolated sense but only in relation to those of another ; and what is paedomorphosis (proterogenesis) and retardation in the one has its complement in hypermorphosis and acceleration in the other. Consequently while there is little likelihood of the full repetition of skeletal phylogenetic stages in ontogeny, except by rare accident, there may well be, and manifestly very often is, a close recapitulation of biocharacter-stages. The genetical approach undermines completely any suggestion of predeterminate phylogenetic control over ontogeny ; but it does not invalidate altogether the old descriptive terms of tachygenesis, bradygenesis, and lipopalingenesis, when they are limited to the changes in bioseries.

The relation of astogeny to phylogeny is much more obscure. The typical zoarium, like that of polyzoans or graptolites, is a clone. Its initial ancestrule or sicular individual is biparental or at least amphimictic ; but all succeeding zooids, being the products of budding, are genotypically uniform. Evolution of the protozooids of each colony may obviously be genetically determined ; but the manner in which the evolutionary changes are transmitted to the whole colony remains unknown, since the ancestroecium of the polyzoans and the sicula of the graptolites characteristically tend to retain their static ' primitive ' form throughout their groups, and in each asty it is the later zooecia or thecae that display evolutionary elaboration. Elaboration of the cup in astogenic growth of the graptolites may be spread over as many as a dozen or a score of thecae before the ' typical ' form is developed ; and in some monograptids Bulman has shown that there is no ' typical ' form, constant modification of the thecae continuing throughout rhabdosomal growth. The analogy of astogenic changes with the ontogenetic changes in non-colonial animals (notably with the ' localised stages of development ' in echinoderms and some arthropods) is superficial, for each colonial individual is (usually if not always) metabolically self-sufficient, and has an ontogeny of its own.

The generalisation that astogeny repeats phylogeny is thus an over-simplification, though a parallelism of structural stages may not infrequently be seen ; and whatever descriptive truth it may hold carries no implification of any kind of the process by which the parallels are brought about. In the phylogenesis of the graptolites the introduction of new thecal types proximally into the asty, and their subsequent extension to distal parts, are likewise called neoteny and proterogenesis only on crude analogy ; while the ' retrogressive ' evolution of thecal form in the later monograptids appears from Bulman's work to be hypermorphic only in a very special sense.

The exceptional modes of growth of the protozoans give foraminiferal ontogeny a status differing from that of either metazoan ontogeny or metazoan astogeny ; and analogy with either is probably both genetically and morphogenetically misleading. Nevertheless, the revealing researches of Wood on *Ophthalmidium* have shown not only that there is some formal relationship between ontogeny and phylogeny in the group (and by implication therefore that there is ground for resting current classification on an ontogenetic basis), but that the nature of the relationship reflects some of the features displayed in more complex animals.

It is evident that there remains a wide field for joint exploration by geneticist and palaeontologist before there can be satisfactory integration of the factors of clonal growth on the one hand and the detailed evolution of colonies or cell-congeries on the other.

In a still wider field the need for the same kind of study is scarcely less obvious. Present knowledge of evolutionary process is mainly based on the tetrapods and a few selected invertebrate lineages. In most other groups, while description of evolutionary stages has been well served, there still awaits a systematic exposition of phylogenesis and its relation to growth and form in populations of living animals.

THE MAINTENANCE AND SPREAD OF SEA-SHORE FAUNAS

Address by

PROF. J. E. SMITH

PRESIDENT OF SECTION D

IT is not surprising, in view of the number and variety of shore animals and the number of environmental situations that are available to them, that shore ecology is, as yet, a relatively undeveloped field of zoology. For, in so far as no two animals have absolute identity of range and habit, every animal poses its own separate ecological problem. Nevertheless, just as there are common elements of structure and of function which make possible a comparative study of morphology and physiology, so there are events and hazards which are the common experience of all shore animals, the course of which determine the numbers and distribution of the several species. In choosing as the subject of my address 'The maintenance and spread of sea-shore faunas' I have in mind to review some of these events and hazards and to examine their consequences in the characterisation of the British intertidal fauna.

There are, essentially, two approaches to the ecology of the shore, one is the method of the local survey, the other the biographical study. In the local survey attention is focussed on the pattern of distribution of individual species and of species-aggregations on a beach or transect sufficiently limited in extent for its broader physical features to be ascertained in some detail. The biographical method neglects the synoptic view of the spatial patterning of adult populations in order, through a study of the biology of individual species, to gain information which the local survey cannot provide. This information is of two kinds which it may be well to exemplify.

The first concerns the relationships of adult shore animals. There are, on our western rocky shores two species of barnacle, *Chthamalus stellatus* and *Balanus balanoides* with an overlapping vertical distribution. In any one geographical locality the extent of the overlap and the relative numbers of the two species are measurable quantities varying from year to year. It would be useless however to attempt an explanation of these quantities merely by reference to one particular region. For one of the barnacles may, in the area surveyed, be at the limit (shall we say) of its temperature range while the other is not; one may prefer exposed shores, the other quiet water; and they may have different substrate preferences. These are the data that must be sought for by tracing the zonation and frequency of occurrence of the barnacles not in one place but over their entire geographical range, and by discovering the conditions under which they breed, and their requirements at all stages of their life histories. The second advantage of the biographical study derives from its attention to the biology of the larval and juvenile phases which, in most instances, spend their lives away from the shore in the open water where they have to contend with physical conditions wholly different from those obtaining on the beach, but which are of primary importance in the replenishment and dispersal of adult beach animals. Some notes on the biology of the common periwinkle (*Littorina littorea*) will serve to indicate the general nature and significance of these conditions and to show how much remains to be discovered of the habits and requirements of even the more familiar of shore animals.

On British shores periwinkles are found wherever there is good holding ground, though they may be rare or absent where the beach is of chalk or limestone or

exposed to storm waves (11). Few winkles are found above the level of the mean high water mark, most of them preferring the middle ranges of the shore (5, 11). Their lower limit is not sharply defined and some individuals seem to live permanently below low water mark. The main breeding season is the late spring and early summer, the encapsulated eggs being shed into the sea at the flood of the tide, usually at night. About a week later the veliger larvae hatch and, after not less than a month tiny periwinkles, with a shell about $\frac{1}{4}$–$\frac{1}{2}$ mm. high and a flat creeping foot, can be taken in a plankton net offshore (20, 35). Some time later (the exact interval is not known) individuals large enough to be visible to the naked eye begin to appear on the shore in some numbers ; here they continue to live and grow, attaining a shell height of 12 mm. or so after one year (22). The larger animals two to three times this length may be several years old.

In giving these facts in brief outline I have not, so far as I know, omitted any major feature of the life history of the periwinkle of which there is a published account. Even so the deficiencies of the story will be evident. It is, for example, common knowledge that winkles are more common at some levels of the shore than others, but no reliable quantitative estimate of their preferred horizon is available. Again, though there are indications that Littorinas in the warmer waters of the south and west coasts of Britain breed over a longer period than those to the north and east, the relation of these events to probable differences in fecundity and availability of new stock has not been enquired into. The possession of pelagic eggs and larvae entails a period of planktonic life and the opportunity for dispersal to new sites by transportation in wind and tidally produced currents, a circumstance serving to remind us how little we are acquainted with the movements of the waters which fringe the shore and in which are contained and distributed the majority of the young stages of oviparous shore animals. In fact when we consider that any larvae drifted seaward into the offshore plankton are probably lost as potential recruits to the shore, the importance of a systematic sampling of the plankton of the marginal waters, as a means of measuring the availability and deployment of the new generations, is so obvious that one wonders why it has been so frequently neglected.

The newly metamorphosed juvenile winkles, if they are to gain the shore, must either be cast upon it by wave and tide or settle on the submerged sea bottom and migrate to the beaches. There are numerous references in the literature on the common periwinkle to the appearance on the shore at certain times of the year of large numbers of young brood but no one has made an accurate tally of the size, numbers and distribution of these youngsters at their first appearance. Bearing in mind the methods used to collect them it would seem that they are not less than about 2 mm. long, and yet when they are settling out of the plankton they are a mere $\frac{1}{2}$ mm. at the most. Where have they been in the meantime, on the shore or making their way in from the sea ? At the moment we do not for certain know the answer to this question, nor for that matter to the more general problem of the manner of invasion of the shore by freely ranging animals with planktonic larvae. An important and productive field of study awaits anyone prepared to make a careful study of the microfauna of the shore approaches and, by the devising of suitable traps, to determine the nature and direction of the onshore movements of juvenile animals of all kinds.

I have considered in some detail this single example of the periwinkle in order to show how the succession of events in a life history brings the animal at every stage of its life into different kinds of environmental circumstances, and how the character first of the one and then of the other environmental feature becomes of consequence in defining the density and distribution of an adult population. I wish now to consider, by reference to a wider range of examples, how the successful maintenance and opportunity for spread of intertidal faunas are contingent upon the way in which individual species, in the various stages of their life history, are able to contend with or take advantage of the external environmental conditions to which they are exposed. In so doing, I shall, of necessity, omit or pass in brief

review some aspects of the problem while considering others in greater detail.

The first requirement for the successful maintenance of a species or population on a shore, and for the opportunity of extending its geographical range, is the production and emission of eggs in sufficient number to ensure against the inevitable losses to which the embryonic, larval and juvenile stages are subject. The fecundity of an animal is, in the first place, an inherent and specific organismal property ranging from the single egg emitted at each spawning by the copepod *Cyclops unisetiger* (4) to the 25 million or so eggs of the edible mussel. But the size of the gonad and the number of eggs contained within it are sensitive to at least one external and variable condition, namely the quality and quantity of food available. In the copepod *Calanus* egg production is directly linked to the amount of food taken, while most animals feed voraciously during the period of build up of the gonads, the body being depleted of its reserves at the time of ovulation when the gonad has made its demands on the rest of the tissues. The relationship between fecundity and food supply has, however, in spite of its importance in the maintenance of stocks, received little attention in respect to shore animals in general, and is an ecological factor well worthy of further study.

If, as would seem probable, food supply is the most important condition of egg production, the release of the eggs at spawning is more directly controlled by temperature changes. Professor J. H. Orton (whose death early this year we so greatly regret), analysed the relationship between temperature and spawning in a great number of marine animals and showed conclusively the importance of temperature changes as a spawning releaser (28). Spawning in most of our coastal animals takes place when the temperature rises or falls to a critical level, the former being the more relevant in warm water species with a predominantly boreal or lusitanian distribution, the latter obtaining for boreal-arctic and high latitude animals with Britain at the southern margin of their range. The great majority of the boreal species of which our shore fauna is chiefly constituted, do not spawn at temperatures below 8° C. but can often be induced to lay eggs in the higher temperatures of a laboratory in advance of the normal time of breeding. This dependence on temperature is further reflected in the onset of spawning at different times at various places along the coast in conformity with local variations in seasonal air and sea temperatures and in relation to the different temperature requirements of different species. The gastropods *Nassarius incrassatus* and *N. reticulatus*, for example, spawn all the year round on all parts of the coast (20). Others, such as *L. littorea*, likewise produce eggs all the year round in the relatively warm waters of the south-west coast (20); on the colder beaches of the Thames estuary, however, spawning may be interrupted in December and January, and in Danish waters there is a similar limitation (35). The conditions of maintenance in British waters are most stringent for species with a distribution centred well to the north or south of Britain. The lusitanian urchins *Paracentrotus lividus* and *Echinus acutus* breed the year round in the Mediterranean. Here their breeding is at the best confined to one or two months of summer and in consequence they retain an occasional, local and precarious foothold in the Cornubian peninsula where the summer temperatures are higher than elsewhere around the coast (25); the holothurian *Labidoplax digitata* with similar requirements is only slightly better established (1). The crab *Dromia vulgaris* is of rare occurrence and its larvae are only occasionally taken in the plankton (19), while even more rare are the sea-hares *Aplysia depilans* and *A. limacina* which would require a succession of very warm summers for the establishment of a breeding stock (15). As a further example there is the common *Octopus vulgaris* whose plague years on the south coast correspond to the hot summers of 1899, 1900, 1911, 1913, 1922, 1948 and 1950. Except in 1899–1900 however when a few embryos were found in the inshore plankton, the population was recruited from embryos drifted over from the Britanny coast (14, 29). An interesting example of a warm water species which can maintain a breeding stock where local temperature conditions are favourable is a variety of *Balanus*

amphitrite found in a number of places where hot, non-toxic effluents cause local heating of the water, and which has almost certainly been introduced from sub-tropical seas on the hulls of ships (3).

In contradistinction to the habits of southern boreal and lusitanian species, arctic-boreal animals with the north and north-east coasts of Britain as their southerly limit might be expected to breed only in the winter months. A number of winter breeding forms such as *Alcyonium digitatum* extend well to the south of this country, but it is not easy to find good test examples of more northerly limited species (41). The winter breeding of the echinoderms *Strongylocentrotus dröbachiensis* and *Cucumaria frondosa* (25), neither of which come south of Scotland, seem however to bear out Orton's principle. Nevertheless for the better understanding of local and more general fluctuations in the number and distribution of shore animals much more information is needed on the influence of ecological situation on the effective fecundity of a species, that is to say the actual number of ova released during the life of an animal. That fecundity does decline as an animal approaches the limit of its range is known in respect to salinity changes; in the Baltic, for example, many of the euryhaline animals of the brackish water fauna are incapable of breeding at their furthest point of penetration (9). Along the British coastline where temperature is the primary factor limiting distribution, the correlation between isotherm pattern, population density and fecundity has yet to be measured. We need to know, in particular, whether the restriction of a spawning period following upon the movement of an animal towards the periphery of its geographical range, or on variations in local climatic conditions, entails a cutting down of the period of emission of eggs but not of the actual number of eggs spawned, or whether both the period and the number are lessened. In either event the future of the brood is to some extent mortgaged, but where the breeding period is short as well as the number of spawned eggs reduced, the additional hazard has to be faced that external environmental conditions may not be favourable over the reduced period of emission for the survival and dispersal of the brood. A longer breeding period is undoubtedly the better insurance policy. In testing the reality of the events I have outlined more information is needed concerning the spawning capacity of animals under conditions which are distinctive and measurable and especially of animals near the limit of their temperature range where their egg production is most critically adjusted. Viviparous species, or species laying encapsulated eggs, would be especially convenient for studies of this kind.

One further aspect of spawning behaviour and its bearing on the distribution of shore animals remains to be mentioned, namely the possibility of an extension of range by the evolution of new varieties and subspecies with different physiological characteristics. Mortensen (25) describes a number of lusitanian echinoderms with Mediterranean and Atlantic varieties distinguished morphologically and by differences of breeding behaviour. Anemones such as *Tealia felina* have a number of varietal expressions associated with different ecological environments and conditions of breeding (34), and similar examples could be found in almost every other major group of invertebrates. The evolution of varieties and subspecies with different physiological characteristics and adapted to different ecological modes greatly increases the opportunities of an animal to extend its range especially when these changes occur at the periphery of its distribution, as in the oyster *Ostrea edulis*, which in the Mediterranean has a number of varieties some of which are adapted for spawning in the direction of high temperature and increased salinity (28). In tracing the extension of an animal's geographical distribution, as in *Ostrea*, or differences of vertical zonation, as in the species or subspecies of the limpet *Patella* (10) or a movement into estuaries, as in the subspecies of the amphipod *Gammarus zadacchi* (32), differences in the time and nature of spawning need always to be examined with a view to the identification of distinct varieties, while the occurrence of visibly different varieties, as in *Littorina rudis*, must always suggest differences in ecological adaptation which, in this particular instance, is indicated by the oviparity of the varieties

patula and *rudissima*, the remaining varieties being viviparous (31).

Spawning in intertidal animals may entail the emission of eggs or early segmentation stages, either free or encapsulated, on to the beach or into the sea under cover of the tide, or their envelopment in a jelly or like material which is subsequently attached to rocks or stones. There may be external or internal brooding of the embryos and, in truly viviparous species, retention of the young in the oviduct until they are spawned as juveniles similar in all respects to the adult except in size and sexual maturity. In a minority of species, of which the starfish *Henricia sanguinolenta* is an example, the breeding habit is facultative (25). Alternatively, species may include varieties with different breeding habits, as in the oviparous and viviparous forms of *Tealia felina* (34).

The varying degrees of retention of, and maternal provision for the young might be expected to carry with them certain advantages and disadvantages in which the need for ensuring the continuity of local populations is measured against opportunities for free dispersal and spread of the species. There is, however, rather surprisingly, no clear evidence that any particular type of early development is especially advantageous in either respect, if we are to judge advantage by the types of breeding practised by the commoner and more widely distributed intertidal species. Most of the shore hydroids, it is true, dispense with the free-swimming medusa, but *Obelia geniculata* and *Clytia johnstoni* do not. Of the three most common shore nemerteans in Britain, *Amphiporus lactifloreus* hatches its young in the crawling stage, *Lineus ruber* has a short free-swimming phase, while *Cephalothrix* larvae may take to the plankton for a fortnight or more (35). Among polychaetes *Nereis virens* has a non-pelagic larva, that of *Arenicola*, though capable of swimming usually stays in the sand (26); the brooded young of serpulids are in the plankton for a short time, while *Eulalia*, *Phyllodoce* and most shore polychaetes have a long larval life (35). *Asterina gibbosa* has a direct development but most of the shore starfishes, brittle stars, and urchins commit their larvae to the plankton (25). Among gastropods, *Littorina obtusata*, *L. rudis*, the dog-whelk *Nucella* and the ubiquitous little *Hydrobia jenkinsi* are either viviparous or have a transitory free-swimming phase, though *L. neritoides* and *L. littorea* have larvae which are in the plankton for a month or more. Nor are breeding habits obviously related to the position of an animal on the beach. The viviparous *L. rudis* is covered by most tides and yet *L. neritoides*, in crevices on the upper shore and rarely wetted, has a planktonic larva (20). Animals in tide pools, in crevices or in other special situations, likewise show a variety of reproductive habit.

The fact that there is no general rule by which we can associate particular kinds of breeding habit with the conditions of shore life would appear to suggest that viviparity, or other circumstances leading to the omission of free living larval stages, with their obvious advantages in the maintenance of local populations are not so inconsonant with population dispersal as might at first sight be supposed, and that oviparity, with the production of planktonic larvae well suited to wide dispersal, may be more than adequate for the conservation of local populations. These suppositions can be put to the test. There is ample evidence, for example, that larvae which are capable of swimming or of being drifted in the plankton will just as readily feed and grow while clinging to the substratum near the site of spawning. Many nemertean larvae have groups of mucus cells at their hind end by the secretion of which they are able to cling tenaciously to sand and stones. *Arenicola* larvae, though taken in the plankton, are from the first well adapted to crawling and are difficult to detach from a solid surface (26). *Axiothella*, *Clymenella*, *Ophelia* (27, 41), and many other polychaetes are in like case and Thorson (35) has calculated that, in Danish waters, about half of the littoral polychaetes in the area are absent, or but rarely represented in the plankton in their early stages. A fairly safe guess might be hazarded that some at least of the free swimming larvae of many shore animals never leave the shore, though one would not wish to include delicate organisms, ill adapted for crawling, such as bipinnariae and plutei, in this category. Nevertheless there seems

to be much room here for a search of pools and puddles by ' panning ' of the sand and overlying water with a fine meshed net in order to discover, identify, and rear to the adult the relatively little known larval fauna of the beaches.

The wide distribution of many viviparous or directly developing species likewise suggests that the young stages are not of necessity bound to a particular situation but may at times be washed from the beaches into the sea. The fact that small crustaceans emerging from their burrows under cover of the tide are drifted offshore by the ebbing tide, and can be taken in the shallow water plankton in considerable numbers is proof of this possibility (36). Direct evidence of a more general dispersal of newly metamorphosed juvenile shore animals is not easy to come by. The observation that tiny recently hatched dogwhelks are always found in the greatest numbers lower down the beach than the adults (23) and that the youngest stages of the pulmonate *Onchidella celtica* are not found at the site of spawning (13) may, however, be indicative of a more generally occurring transportation of juvenile stages which neither viviparity nor the protection of the young in other ways is able wholly to counteract.

There is of course no doubt that in opportunity for wide dispersal the advantage lies with animals having a long lived planktonic larva or a pelagic embryo. *Octopus* embryos are drifted across the Channel almost every year and may reach the coast of Denmark some 400 miles away (29). The Californian shore crab *Emerita* is taken in its later larval stages more than 100 miles out to sea (9) and larvae caught up in the Gulf Stream and other major oceanic currents must travel even further.

It is probable that in British waters most larvae live for about four weeks, though the period ranges from a few hours to a few months. For any given species it is shorter in summer than in winter, and in many instances larval life can be prolonged if the larvae, on the approach of metamorphosis, are unable to attach or to find a substratum suitable to them. Larvae capable of self-determined movements tend, as they get older, to avoid the better lighted surface waters and, by sinking in the water, are given opportunity for settlement. To this extent they determine for themselves the conditions of survival. But for shore animals a more serious hazard is the arrival of their larvae when ready for metamorphosis at the approaches to the shore, and this, for weak swimmers, is a fortuitous happening dependent upon the movements of the water masses in which the larvae are carried. Let us for a moment consider the general nature of these movements first with respect to the possibility of introduction of foreign species and secondly to the maintenance and spread along our coasts of established forms.

The prevailing and dominant south-westerly winds from the Atlantic produce an extensive north-easterly drift of water which, on reaching the south-western approaches, is deflected into two streams. The one bears northerly to the west of Ireland whence, after rounding Cape Wrath, it flows parallel to the coast of Norway into high latitudes ; the other flows into the Channel, the surface drift persisting through the Straits of Dover at most times of the year to fetch up on the continental coast as far north as the Skagerak.

The North Sea, to a large extent isolated from the North Atlantic drift, is open to the north to sub-arctic water which enters mostly at deeper levels. The colder water mixing with deflected and warmer drift water feels its way southward down the Scottish and Northumbrian coast to the neighbourhood of Flamborough where it turns eastward to form the boundary between the northern and southern basins of the North Sea. The coast between Flamborough and the Thames estuary is the western boundary of the latter basin, the waters of which, though replenished to some extent from the Channel and from the northern basin, form a more or less self-contained region.

There are thus two major thrusts of water against our coasts. That from the south-west is the way of entry of boreal and lusitanian species, while the northern passage into the North Sea allows of the possibility of entry of arctic-boreal and arctic forms. The former drift is the stronger and the more sustained and is varied chiefly in its effects upon our coastal fauna by fluctuating hydro-

graphical conditions in the Celtic Sea to the south-west of the Scillies (6), which fluctuations allow at times a greater influx of oceanic water into the Channel and Irish Sea and at others the passage of water which is predominantly Biscayan in origin. Oceanic water can have but little to contribute to a permanent coastal fauna, its immense fetch precluding the introduction of intertidal animals either as adults or larvae though pelagic species of the Atlantic open water are, as Russell (30) has shown, important indicators of the presence in the Channel of this outside water. The Biscayan water on the other hand skirts the Iberian peninsula and the French coast in its northward passage bearing with it, we may suppose, larvae and embryos shed into the sea along the length of this part of the eastern Atlantic seaboard. It is to be expected that a better knowledge of the conditions which govern the entry of this biologically important body of water into the Channel will follow upon further analysis of the hydrography of the Celtic Sea.

The lusitanian and southern boreal fauna diminishes in number and variety to the north and east of the Cornubian peninsula with the falling temperature of the coastal water, arctic-boreal introductions not being of sufficient number to make good the loss. Northern forms, though perhaps as well adapted physiologically to thrust southwards into our waters as the southern forms are to extend northwards have greater physical difficulties with which to contend in their passage. Whereas Land's End is not much more than 100 miles from the French coast, the north of Scotland is separated from southern Scandinavia by some 300 miles of sea. The Biscayan species, moreover, have a favourable current to assist them in their passage, whereas larvae from northern waters meet with a contrary surface drift and, if they are to move southwards, must perforce enter slowly moving, deeper currents. It is indeed doubtful whether, under present-day hydrographical conditions, any strictly intertidal arctic-boreal species such as *Balanus balanoides* can make the direct sea passage from Norway to the Scottish coast, though the journey would present less difficulty to animals like *Solaster endeca*, a northern starfish tolerant of both shallow and deep water which extends southwards along our east and west coasts but does not enter the southern North Sea basin or the Channel (25).

In very recent historical times, however, and I mean by that the 150 years or so during which the British marine fauna has been intensively studied, few animals have succeeded, by a natural extension of their range, of gaining a foothold on British shores, and it is doubtful whether there is a single unequivocal instance of such invasion leading to permanent establishment. Such animals as can be expected to gain a permanent foothold here have had a relatively long period in which to make trial of the hydrographical tests to which invading species are now subject. The spectacular success of artificially introduced species such as the New Zealand barnacle *Elminius modestus* (2) and the American molluscs *Crepidula fornicata* and *Petricola pholadiformis* (41) shows, however, that many animals would find the conditions here congenial if their larvae could but make the journey. It would seem safe to conclude that, unless the hydrographical conditions in the eastern Atlantic greatly change, new natural introductions will continue to be few and substantially limited to the extreme south-west, with the southern North Sea basin the region the least accessible and the most hostile to invaders. Naturally occurring additions to the British littoral fauna seem most likely, in the absence of marked climatic changes, to appear in the form of new and locally differentiated species, a possibility made real by the recent discovery in the Clyde of a number of polychaetes sufficiently different morphologically and in breeding habit from closely related and more widely ranging species to be worthy of specific recognition (4a).

If, however, the hydrographical setting and zoogeographical position of Britain does not at the present time greatly encourage the natural introduction of outlying species, the movements of the coastal surface waters appear, on general inspection of the hydrographical and meteorological conditions, to favour the conservation and longshore distribution of the resident intertidal fauna. The necessary confirmation of this through comparisons of the content of inshore and

offshore plankton is not easy to come by, though none of the available information is contrary to the expectation. At Plymouth, where records of the character of the inshore and off-shore plankton are more complete than elsewhere, the plankton of the Sound is markedly richer in the larvae of the commoner intertidal animals than is the plankton from outside the Breakwater. It is, for example, in the appropriate season, rich in barnacle nauplii and eggs of the shore Littorinas both of which are comparatively rare outside. The veligers of *Nassarius reticulatus*, confined to the shore and littoral fringe, are only to be found in the inshore plankton while those of the deeper water *N. incrassatus* are as common offshore as inshore ; there is a like distinction in the distribution of the larvae of the shore *Porcellana platycheles* and the more widely ranging *P. longicornis* (21).

Conditions in the land-locked waters of Plymouth Sound are admittedly more favourable than on a more open coastline for the retention, in the marginal waters, of shore produced young. More extended observations on the planktonic distribution of the larvae of shore animals are clearly needed both as a measure of the movements of the inshore surface waters and of the local conditions of recruitment of intertidal animals. In the meantime it is not unjustifiable to suppose that the waters fringing the shore transport, in the appropriate season, great numbers of pelagic embryos and larvae which, as they are deployed along the coast, await the opportunity for metamorphosis and settlement. Wilson, and Day (7, 38), Thorson (35), and other workers have shown how critical the substrate requirement is for the metamorphosis of many species depending, so it appears, on the texture and chemical constitution of both particulate and solid surfaces. Although the correlation has not been fully established, it would seem that animals that are free to wander after metamorphosis may be the more tolerant, and there is much circumstantial evidence to show that in many instances they metamorphose offshore, later migrating to the beaches. The first young crab stages of the common shore crabs *Carcinus maenas* and *Portunus puber* are, for example, either buoyed up in the plankton or are on the sea floor below low water mark, and the second young crab stages must be sought for under stones at low tide (19). Again, the youngest stages of nudibranchs and other gastropods are not normally found on the beach while the smallest specimens of a beach population are taken at lower levels than the adults (20). It is among the animals which do not change their position after metamorphosis, or whose movements are strictly localised, that the substratum requirements for metamorphosis are most evident. The texture preferences of a number of polychaetes have been demonstrated and analysed (7, 38), and barnacle cyprids have been shown to seek out broken surfaces, and, in particular, surfaces which bear or have borne other barnacles (18). This argues a chemical attraction which can hardly be denied in the pairing off of commensals and which perhaps is seen in its most selective aspect in the settling of the cyprids of the barnacle *Pyrgoma anglicum* which is usually to be found on the calyx of the Devonshire cup coral *Caryophyllia*. There can be no doubt whatsoever that one of the most important and as yet little investigated factors in the maintenance and distribution of shore animals is to be sought in the substratum requirements and behaviour of metamorphosing larvae at the time of their deposition on the shore or at the shore approaches.

With the arrival on the shore of the metamorphosing larvae or of shoreward moving juveniles, the phase of offshore pelagic dispersal ends and the animal enters upon a period of rapid growth and development to maturity. In the early stages of shore life mortality is often very high, as many as 80–90 per cent. of the young of the common periwinkle, for example, dying under the rigours of a changeable and unaccustomed environment during the first year (22). A high juvenile mortality is of course to be expected. With sessile animals unable, once attached, to change their position, conditions favourable at the moment of attachment may turn out in the long run to be unfavourable as when, for example, the cyprids of a barnacle which, when adult, is found only on the upper reaches of the shore, settle at a lower level ; the adult zonation is then achieved by the elimination of all the young outside the

area of adult distribution. Animals free to wander attain their adult distribution by searching for favourable situations ; here again, if these are long denied them, many of the juveniles will die.

Once a population has gathered in an area favourable to it a more stable equilibrium is reached. Thereafter carnivores like the dogwhelk feeding upon barnacles, starfishes upon mussels, and nudibranchs browsing on hydroids and sponges, take a steady and not inconsiderable toll, the mortality consequent upon these and other causes creating in a population of long lived animals a progressively declining proportion of the older age groups. The mortality among adults can, on occasions, be catastrophic as when, for example, whole populations of the lamellibranch *Tellina tenuis* are wiped out in exceptionally cold winters (41) but, by and large, the death rate among adult shore animals is a predictable statistic.

These temporal fluctuations are often accompanied by spatial changes in the distributional pattern of an adult population. Vast numbers of the hopper *Talitrus* are frequently to be seen moving up the beach before the advancing tide (37) ; burrowing crustaceans emerging from the sand are swept from one place to another by the ebb and flow of the water (38) ; shore crabs and the larger crustaceans migrate up and down the beach at different seasons of the year, while gastropods and other animals often travel considerable distances whenever the rock or sand on which they are living is wet enough for movement to be sustained. The restless movement of so wide a variety of animals creates the impression of continuous change in the number and distribution of the fauna. But it would be unwise to emphasise the magnitude of these changes or to suggest that they substantially alter the distributional pattern. One of the most outstanding characteristics of a shore fauna is the persistence with which individual species and species-associations adhere to situations for which they have a preference. The fauna of a crevice is predictable if its geographical location and vertical position are known, and on an open beach most species have a vertical extension the upper and lower limits of which are determinate and more or less fixed. Other animals are tied to particular qualities of substrate, or are associated by preference or obligation to a particular animal or alga, or to an algal or animal association.

The movements of adult shore animals, though rarely checked for any length of time, and necessary in the search for food and maintenance of shelter, are thus, so long as they are self-determined, designed not for wide dispersal but for the maintenance of their appropriate habitation. They cease, moreover, when the animal is subject to the action of waves or to other circumstances tending to dislodge or transport them. At such times animals remain in the shelter of stones or weed or bring into action organs of adhesion, and though there are, no doubt, occasions when the force of the water is not to be resisted, there is no evidence to suggest that they are sufficiently common or so long maintained as to bring about population displacements large enough to operate as an effective means of dispersing the species. We are thus confirmed in the conclusion that the events which govern the spatial and temporal fluctuations in the pattern of a shore fauna, and the appearance or absence of a particular species on a beach, are played out in early life and, in most cases, far away from the adult territory.

These are the events the nature and significance of which I have discussed in some detail and to which I now return in conclusion in a brief consideration of their bearing on the regional zoogeography of the British intertidal fauna.

If a line be drawn from east Dorset to Flamborough the coastline west and north of this line is almost wholly bounded by Palaeozoic and Mesozoic rocks forming rocky headlands and beach platforms, and eroded, in the softer strata, into bays and creeks floored by sand or other particulate material ; only in limited stretches of this coastline is the rock concealed by drift deposits giving rise to low-lying unstable cliff and beaches devoid of good holding ground. To the east and south of the Flamborough-Dorset line on the other hand, boulder clay or soft Tertiary deposits predominate so that the greater part of the coastline of the southern North Sea and of the eastern Channel coast is

bordered by shingle, sand, or muddy beaches which at their worst are virtually azoic and at the best are unable to support a rich and varied epifauna. The climatic and hydrographical conditions accompanying the two types of coastline emphasise the distinctiveness of their associated faunas. The rocky coastline from Dorset westwards and northwards to the Pentland Firth is bathed by water of Atlantic origin which, though varying locally in its content of nutrient salts and no doubt in other properties, shows a smoothly graded temperature fall in the direction of its drift. The presence of this water makes of the west coast as a whole a well defined faunistic region in which the predominating boreal species suffer, in the different latitudes, a varying admixture of southern or of northern forms. The coasts of eastern Scotland and of north-east England, though similar in topography and lithological character to those of the western seaboard, are bathed by water made up in part from a southerly deflection of the North Atlantic drift and in part from colder northern water entering the North Sea at the surface and at depth. The faunistic changes consequent upon the changed hydrography are not spectacular but the rather scanty information available shows them to follow the expected course of the deletion of a number of southern boreal species such as the barnacle *Chthamalus stellatus* (24) and the introduction of northern forms, in particular those animals which, like *Strongylocentrotus dröbachiensis*, affect a wide bathymetric range (25), enabling them (we might expect) in the course of successive generations to make the crossing from Scandinavia to Scotland. Some few species, including the urchin *Strongylocentrotus*, the starfish *Porania pulvillus*, the sea-cucumber *Cucumaria frondosa* and the anemone *Phellia gausapata* (25, 34), do not extend far south but the intertidal fauna remains of much the same general character along the north-east coast to Yorkshire and the neighbourhood of Flamborough.

South of the Flamborough promontory, however, marked changes occur and the fauna becomes much less varied. Among echinoderms, for example, some twenty species taken to the north on the shore or at depths not exceeding ten metres are absent from the southern North Sea littoral (25). The anemones *Anemonia sulcata* and *Cereus pedunculatus*, common to the north, are no longer to be found (35), and in every group there is a similar discontinuity to the detriment of the more southerly fauna. This state of impoverishment persists through the Straits of Dover along the south coast as far as the Isle of Wight and possibly a little further. West of this most of the animals excluded at the Flamborough divide reappear and to them are added many of the southern boreal forms such as *Chthamalus* and the more occasional members of the lusitanian element, the faunistic break at the Isle of Wight divide (12, 25) being, in consequence, even more startling than at Flamborough.

The restriction of the fauna between Flamborough and the Isle of Wight, coinciding as it does with a shore line of predominantly particulate beaches with few places where there is a platform of holding rock, would naturally suggest that the paucity of the fauna is a direct consequence of the prevailing and relatively monotonous beach structure. Further consideration shows, however, that this simple explanation will not suffice. There are, after all, opportunities, though admittedly restricted, at various places along the coast for the attachment of an epifauna. Anyone who has inspected piers and harbour structures at Lowestoft, Southwold and elsewhere along the East Anglian coast will know how common at times are *Alcyonium digitatum*, *Metridium senile* and other attached animals, while the more stable beaches, especially where there are mussel beds, support starfishes and brittlestars of a restricted number of species in quantity. Moreover it is not only on the beaches that the impoverishment of the fauna is made manifest. Animals unable to pass the barrier along the coast are equally unable to intrude along the deeper sea floor, a circumstance that would seem to rule out air temperature with its greater range and lower winter minimum on the east coast as a limiting factor in all cases. Nor would it appear that there is any hydrographical barrier wholly preventing the entry of animals either as adults or as larvae into the southern North Sea basin ; if there is it acts in a curiously selective manner,

allowing the passage shall we say of *Asterias rubens* but not of *Marthasterias glacialis*. Sea temperature can also be dismissed as a factor likely of itself to lead to the observed limitation. In the North Sea the offshore summer and winter water temperatures are neither higher nor lower than those met with by many of the excluded animals at other places within their range. Similar considerations apply to salinity, it being worthy of note in this connection that shore animals are exposed on most shores to greater salinity variations than are experienced by those of the open sea and that, in consequence, however relevant salinity may be in the distribution of pelagic and benthic organisms they are as a rule less restrictive to intertidal animals. The waters of the eastern English Channel and of the North Sea are not everywhere and at all times poorer in their content of phosphates and other nutrient salts than the open sea Atlantic water, though the latter is subject at the continental shelf to enrichment on a greater scale by upwelling of deep nutrient rich water into the surface layers. It is doubtful however whether the presence or absence, or the scarcity or abundance of intertidal and shallow littoral animals can be satisfactorily correlated with variations in the content of nutrient materials in the offshore waters ; if any relationship exists it has, to say the least of it, yet to be established. So far as we know the supply and the cycles of regeneration of nutrient material on the shore and in the marginal waters may be, and probably are, largely detached from the events of the deeper offshore waters.

There would thus appear to be no reliable evidence pointing to the fact that shore faunas washed by North Sea water are poorer than those of the Atlantic seaboard because of the greater or lesser abundance of phosphates, nitrates or other nutrient materials present in measurable quantities in the one or the other. And yet, to recapitulate, there is a difference in the quality and quantity of these faunas which is not wholly to be accounted for by differences in the physical nature of the two shorelines but which seems to be coincident with the two types of water.

The recent work of Dr. D. P. Wilson (39) in which he shows that larvae of a few selected shore and shallow littoral animals develop more successfully in oceanic water from the Celtic Sea than in English Channel water, draws from him the tentative conclusion that the Channel water lacks or is deficient in some unidentified constituent essential for development, and that many of the biological changes that have taken place in recent years in the Plymouth area, which is near the boundary of the insurgent Atlantic water, would be explicable in terms of known hydrographical changes if the presence or an unknown but essential principle in the oceanic water be allowed.

It is, I think, fitting that I should conclude my review of some of the conditions by which intertidal faunas are maintained and are able to spread with an admission of our ignorance of many of the conditions which affect the development of larvae, and are therefore of primary importance in the distribution of littoral species. This particular mystery serves to emphasise the unity of marine biological studies in which, as I have tried to show, the natural history of shore animals can no longer be regarded as a detached field of investigation but as one aspect of marine biology to be understood only in relation to knowledge made available through wider oceanographical studies, and that the natural history of shore animals in its turn has its own private information which the oceanographer cannot afford to neglect in his search for data which will throw light on the movements and properties of the medium which is his special study.

References

(1) —— (1931). *Plymouth Marine Fauna.*
(2) Bishop, W. H. M. (1947). *Nature*, **159**, 501.
(3) Bishop, W. H. M. (1950). *Nature*, **165**, 409.
(4) Calman, W. T. (1909). *A Treatise on Zoology.* Part VII. Crustacea. A. & C. Black.
(4a) Clark, R. B. (1952). *Nature*, **169**, 975.
(5) Colman, J. S. (1933). *J. Mar. Biol. Assoc.*, **18**, 435.
(6) Cooper, L. H. N. (1952). *J. Mar. Biol. Assoc.*, **30**, 465.
(7) Day, J. H. & Wilson, D. P. (1934). *J. Mar. Biol. Assoc.*, **19**, 655.
(8) Eales, N. B. (1939). *The littoral fauna of Great Britain.* C.U.P.
(9) Ekman, S. (1953). *Zoogeography of the Sea.* Sidgwick & Jackson.
(10) Evans, R. G. (1947). *Proc. Zool. Soc. London*, **117**, 411.

(11) Evans, R. G. (1947). *J. Mar. Biol. Assoc.*, **27**, 173.
(12) Fischer-Piette, E. (1936). *J. Linn. Soc.*, **40**, 181.
(13) Fretter, V. (1943). *J. Mar. Biol. Assoc.*, **25**, 685.
(14) Garstang, W. (1900). *J. Mar. Biol. Assoc.*, **6**, 260.
(15) Grigg, U. M. (1949). *J. Mar. Biol. Assoc.*, **28**, 785.
(16) Huntsman, A. G. (1918). *Trans. Roy. Soc. Canada*, 12.
(17) Kemp, S. (1939). *Ann. Rep. Brit. Assoc.*, 85.
(18) Knight-Jones, E. W. & Stevenson, J. P. (1950). *J. Mar. Biol. Assoc.*, **29**, 281.
(19) Lebour, M. V. (1928). *Proc. Zool. Soc. London*, 473.
(20) Lebour, M. V. (1937). *J. Mar. Biol. Assoc.*, **22**, 105.
(21) Lebour, M. V. (1945). *J. Mar. Biol. Assoc.*, **26**, 527.
(22) Moore, H. B. (1937). *J. Mar. Biol. Assoc.*, **21**, 721.
(23) Moore, H. B. (1938). *J. Mar. Biol. Assoc.*, **23**, 67.
(24) Moore, H. B. & Kitching, J. A. (1939). *J. Mar. Biol. Assoc.*, **23**, 521.
(25) Mortensen, Th. (1927). *Handbook of the Echinoderms of the British Isles*. O.U.P.
(26) Newell, G. E. (1948). *J. Mar. Biol. Assoc.*, **27**, 554.
(27) Newell, G. E. (1951). *Proc. Zool. Soc. London*, **121**, 561.
(28) Orton, J. H. (1920). *J. Mar. Biol. Assoc.*, **12**, 339.
(29) Rees, W. J. (1950). *J. Mar. Biol. Assoc.*, **29**, 361.
(30) Russell, F. S. (1936). *Conseil Intern. Rapp. et Proc.-Verb. des Réunions*, **100**, 9.
(31) Seshappa, G. (1947). *Nature*, **160**, 335.
(32) Spooner, G. M. (1947). *J. Mar. Biol. Assoc.*, **27**, 1.
(33) Steers, J. A. (1946). *The Coastline of England and Wales*. C.U.P.
(34) Stephenson, T. A. (1928, 1935). *The British Sea Anemones*. Vols. I and II. Ray Society.
(35) Thorson, G. (1946). *Medd. fra Kommiss. fra Danmarks Fiskeri*, 4.
(36) Watkin, E. E. (1941). *J. Mar. Biol. Assoc.*, **16**, 81.
(37) Williamson, D. I. (1951). *J. Mar. Biol. Assoc.*, **21**, 91.
(38) Wilson, D. P. (1948). *J. Mar. Biol. Assoc.*, **27**, 723.
(39) Wilson, D. P. (1951). *J. Mar. Biol. Assoc.*, **30**, 1.
(40) Wilson, D. P. & Armstrong, F. A. J. (1952). *J. Mar. Biol. Assoc.*, **31**, 335.
(41) Yonge, C. M. (1949). *The Sea Shore*. Collins.

THE GEOGRAPHER AS HUMANIST

Address by

PROF. R. H. KINVIG

PRESIDENT OF SECTION E

BY a curious twist of fate I find myself occupying the Presidency of Section E at the place which is inevitably associated with the work of Percy Maude Roxby who had such a distinctive influence on the growth of geography as a subject of full academic stature for both research and teaching; and I propose to direct your attention to some of the outstanding characteristics of his approach to the subject in the light of subsequent trends in geographical thought. My own link with him goes back to the years before the First World War, and before the establishment of the honours school of geography, when, as an honours modern history student, I was permitted to attend all the geography courses then available. Later on, during session 1918–1919, I became his chief assistant at the time when the geography honours school was in its initial period of growth.

Roxby was born in 1880 in Buckden, Huntingdonshire, and he started life with a deep attachment to the rural tradition of England which remained with him throughout, despite his close contact, later on, with city life in Liverpool as well as with the outer world. After attending Bromsgrove School, he read modern history at Christ Church, Oxford, obtaining a first class in 1903. In all probability he had attended some of Mackinder's lectures while still studying history—most of Mackinder's audience were historians—but it was not until session 1903–1904 that Roxby became a student in the Oxford school of geography; and so, just fifty years ago he was listening to both Herbertson and Mackinder while preparing for the diploma in geography. From both of these pioneers he learned much, but it was Herbertson who made the deeper impression on him, and it may be recalled that he referred to Herbertson as his 'honoured master' in the presidential address he delivered to Section E in 1930 (1). He did not stay long enough to complete the course since, on Herbertson's recommendation, he went to Liverpool in October 1904 as an assistant lecturer in geography in the economics department, under Professor E. C. K. (later Sir Edward) Gonner. Thus began his long connection with the University of Liverpool that lasted until 1944, during which period he became, successively, lecturer (in 1907), and then Professor of Geography on the creation of the chair in 1917. Never did a university have a more devoted, sincere, or loyal servant, and never did a geography department have a more inspiring head. He never spared himself, and he built up what came to be acknowledged as one of the finest departments of geography in the country.

Before coming to the most crucial stage in the development of that department, which actually came in 1917 with the establishment of the honours school, there are two points to which I must refer. One is Roxby's journey round the world as the Albert Kahn Travelling Fellow in session 1912–1913, during which he visited several European countries, then India, South-east Asia and China, and finally the United States and Canada. This experience made a very deep and lasting impression on his geographical ideas as may be judged by reference to the report he published on his return. One of the most fascinating sections in this is his analysis of the conflict between eastern and western cultures as seen in India and the Far East (2); and it was this visit to China which marked the beginning of a life-long attachment and devotion to that country and its people. It was there that he died

in 1947, having resigned from his Chair in 1944 to go to China as the chief British Council representative. The other significant event was the outbreak of the First World War in 1914 which hurried on the development of geography at Liverpool. Roxby, as an essential part of his war work, threw himself with all the strength of his mind and body into the work of lecturing on the geographical aspects of the war—on its causes and on the hopes to be realised after its successful conclusion, hopes which were more difficult to realise than was then fully appreciated. He was a tall, impressive figure with a voice which carried well, and he had a distinctive manner of lecturing. He drew large audiences to the Arts Theatre and held them enthralled while he made the wall-maps live as they had never done before. Those lectures were a revelation of what geography could be.

It was then in 1917 that the honours geography degree of B.A. came into being, and Liverpool has the distinction of being the first university in this country to establish such a course, although it was quickly followed by Aberystwyth under Professor Fleure, and by London. Looking back on the honours scheme worked out in Liverpool in those early years it may ustifiably be claimed that it was an essentially well-balanced one, providing (i) general or systematic courses in both physical and human geography, the latter being a particularly significant feature ; (ii) a complete course on the historical geography of the British Isles ; (iii) regional studies of areas such as China and the United States, in which both physical and human aspects were studied, while as much as possible of the historical geography was included as an essential background to the present human geography ; and (iv) cartography. There can be no doubt that the general plan has justified itself as shown by its relative stability, while it has also permitted developments along particular lines. Since then, the growth of the subject in universities within Britain and throughout the Commonwealth and Empire has been most impressive ; but what I particularly wish to examine is the content of the subject.

There are still, unfortunately, some people (usually older ones) who think of geography as being merely ' location ' in its most elementary sense, and such an attitude survives particularly in various Government publications, e.g. the Colonial Reports. In these the form generally adopted is that after various chapters on population, productions and other aspects of economic and social conditions, there is a section, usually very dull and formal, labelled ' Geography and Climate,' the latter being distinguished from the former. There can surely be little, if any, justification nowadays for such an attitude. Amongst geographers themselves there has been much hard thinking about the nature and scope of their subject and there is now much more general agreement. It might, indeed, be said that most could agree to accept the same definition although this would not mean that they would agree about its interpretation. This is perhaps inevitable ; and, provided there is tolerance on all sides, these differences can be regarded as a source of strength rather than as a matter for alarm. It is useful, nevertheless, to look at some of these differences in interpretation and to that aspect I should next like to turn.

One of the notable steps forward made recently by the Committee of Section E was the appointment of a Geographical Glossary Committee in 1950. This Committee has grappled boldly with some vexed problems of terminology, and it has published various lists of suggested definitions in the *Geographical Journal*. Quite apart from its detailed achievements in helping to solve problems of definition, this Committee has, I feel sure, succeeded in bringing before geographers themselves, as well as the wider public, the need for greater precision in our use of various terms.

The definition of geography proposed by the Glossary Committee is ' the science that describes the Earth's surface with particular reference to the differentiation and relationship of areas ' (3). This ingeniously tries to combine the *Oxford Dictionary* definition, contained in the first part of the statement, with another shorter definition given by the American, Professor Hartshorne, in his now classic survey of the *Nature of Geography*, viz. ' the study of the areal differentiation of the world,' or, if we wish to express it in

another form and also revive a term common in former days, we can describe it as 'chorology, the science of regions' (4). My own preference is for either of the latter, but we should first concentrate on the other definition where the point at issue becomes the interpretation of the phrase 'the Earth's surface.' Is it to be taken only in its more literal and restricted sense of what can actually be seen on the surface—rocks, soils, patterns of cultivation, village and urban morphology—or can it, indeed must it not, also include some further study of man himself, and his ideas and institutions which are not necessarily recorded on the actual surface? There seems no doubt that the former view is the one favoured by the Glossary Committee which starts off its list of definitions with one explicitly on 'Earth's surface' where it is stated rather tautologically that 'while geography is immediately concerned with the surface of the Earth, it is customary to include the atmospheric envelope and the Earth's crust in the term Earth's surface in so far as they directly affect the surface.'

The effect of this point of view is to make geography primarily a physical study having as its field the analysis of physical conditions (or perhaps we ought to use the term 'natural' so as to include biogeographical factors in addition to those of relief, structure and climate), followed by the effects of these conditions on man and his works. A consequence is that 'geographical' becomes synonymous with physical, and there can be no doubt that this is the way in which the term is still used by the vast majority of people at any rate in Britain.

I appreciate, let me hasten to add, that there are many whose dominant interest is on the physical side of geography who concede the existence of a human geography defined by the Glossary Committee as 'the geographical study of the features, objects and phenomena of the Earth's surface which relate directly, or are due, to man and his activities.' In this context Professor Wooldridge has maintained that man is indeed one of the chief differentiating agents 'worthy at least to rank with tectonic forces, erosional processes, climatic conditions and the rest' (5), but I am convinced that any attempt to circumscribe the subject can, ultimately, only produce harmful results in making human geography a perfunctory or even nugatory study.

We may illustrate one of the more devastating results of this kind of approach by reference to the argument, still all too commonly used, that the effect of modern developments is to wipe out geography altogether! Thus in a short study on *What is a Nation?* published by the Royal Institute of International Affairs in 1945, the essential theme of its author, Mr. Stannard, is that 'in the modern world, the geographical factor is of diminishing importance.' Both the United States and Canada are nations constructed 'in defiance of geography,' for, he argues, 'the geographical divisions of the North American continent run vertically from north to south. The two nations which occupy the greater part of it stretch from east to west. Their frontiers have been carried from the Atlantic to the Pacific by a process of expansion governed throughout by the human will whose strength has vanquished geography' (6). The most chastening aspect of this matter lies, to my mind, in the fact that some geographers actually subscribe to such a doctrine. But of course Roxby would not have accepted such a view, nor can anybody who caught the spirit of his attitude to human geography. What is this attitude? Briefly, man himself is an integral part of the geographical synthesis, man and his thoughts, traditions and aspirations in relation to the actual land in question have to be weighed against the supposed dictates of the 'geographical divisions' (by which are usually meant 'physical divisions') as urged by Stannard and those who think like him. Furthermore, Canada and the United States *are* geographical realities just as surely as are the physical divisions with which they do *not* conform, though clearly of a different kind. It is no doubt disconcerting to the mathematical mind that there should be a discordance between the patterns formed by these two sets of geographical phenomena, but such discordance is not unique, and like all other cases, it is a worthy subject for study.

The burden of our argument is, then, that man must form part of geographical

study, and I urge this despite the powerful support given for a rather contrary view expressed by Wooldridge and East in *The Spirit and Purpose of Geography* and by the first of these authors in his stimulating survey of *The Geographer as Scientist*. They call to their aid an almost identical phrase from the writings of two acknowledged masters of our craft, Vidal de la Blache and Carl Sauer, to the effect that geography is ' the science of places not of men ' (7), and this has been accepted as conclusive by some others such as Edwards (8). Let us willingly accept the statement as a true statement of principle, but let us also interpret it in the spirit in which it was applied by the two honoured exponents of our subject. We might first pursue the same argument to affirm that geography is the science of places and not of rocks, or plants, or air masses, but that line of thought would hardly justify the conclusion that the geographer is not thereby vitally interested in the subjects of geology, botany or meteorology. The real point, surely, is that all of these features are essential contributory elements in building up our conception of any place, and so is man in his many roles, who gives such a distinctive character to any particular place. I submit that this is the more helpful spirit in which to view the quotation in question. As far as Carl Sauer is concerned I have some considerable assurance that he would not accept the approach attributed to him, and this is in keeping with his varied writings. Consider, for example, his assertion in one of his latest studies that ' an environment can only be described in terms of the knowledge and preferences of the occupying persons : " natural resources " are in fact cultural appraisals ' (9). In a similar vein Preston James and Kline insist that while ' human societies must adjust to the qualities of the land they occupy, the method of adjustment is determined by forces within the society, biological and cultural ' (10). After all, among our own British pioneers, Herbertson expressed views which are in essential harmony with these although he did not live long enough to work them out thoroughly. Despite the fact that his name is linked primarily with that of natural regions, Herbertson was deeply impressed with the need for systematic studies in the human sciences—anthropology, ethnology, sociology and political institutions—on which he was engaged at the time of his death ; and in a paper published posthumously he showed clearly enough the direction of his thoughts. Urging the need for studying man in relation to his environment he wrote, ' there is a whole for which we have no name, unless it is a country, of which men are a part. We cannot consider men apart from the rest of the country, nor an inhabited country apart from its inhabitants without abstracting an essential part of the whole ' (11).

Roxby, too, fully appreciated that the geographer's ' unit is the place or region ' ; but he had a much wider conception of human geography than is usual, and from what was said earlier on there can be little doubt that he derived from Herbertson many ideas which he continued to elaborate. Human geography is, then, something more than the mere analysis of the influence of physical factors upon human groups. To take only one or two examples, how else can we hope, or pretend, to make any sane approach to the study of, say, Northern Nigeria where the Hausas and the Fulani, the two dominant human occupants, exhibit many essentially distinct characteristics not only in their physical appearance but also in their cultural make-up and their economies, and yet they co-exist under the same set of natural conditions ? On the contrary, the Yorubas of South-west Nigeria, although occupying at least two vegetation types—the forest and the savanna—exhibit some fairly uniform cultural characteristics, such as their types of building, and the fact that men take a greater share in cultivation than is customary in other parts of equatorial Africa. We need to take a broader as well as a deeper view of the various human groups in their regional diversities, and as influenced not only by their physical environments but by the nature of their own adjustments to those environments. In this connection Roxby distinguished four principal aspects of human geography, namely, racial, social, economic and political, and argued that there should be as intimate a study of the relationship between these, from the geographer's standpoint, as between the

different aspects of physical geography. In other words there is a human environment in addition to the physical or natural environment, and it is this ' holistic ' view of our subject which, although, as far as I am aware, Roxby never used the term, is the one I would commend as being more satisfying mentally and more fruitful in its results than that which permits such a title as, e.g., ' the geography and peoples of Russia.' The very use of such a phrase reveals to the full the old dichotomy between man and nature against which so many geographers have fought in the modern period from Humboldt and Ritter onwards. In connection with this it is very pertinent to recall that only two years ago, H.R.H. The Duke of Edinburgh, in opening the business of Section E on the occasion of its centenary, emphasised the need for geographers to get to know as much as they can about the world and its peoples, for, as he put it ' getting to know about each other *is* geography ' (12).

The suggested grouping of the data of human geography under the conventional categories of racial, social, economic and political, provides a useful working scheme although it cannot be adhered to rigidly, and there is constant overlapping between these factors. Moreover, there is no strict relationship between these groupings and the well-known diagrammatic representation of the several systematic sciences as shown by Hartshorne, following Hettner (13). There is, however, general agreement with that scheme, the only real difference being that Hartshorne includes racial geography, along with plant and animal geography, under the biological group as separate from the more specifically social or human branches. Logically, this is correct and it is salutary for all to realise, at the outset, the implication of this ; yet in so far as the racial aspect is essential in giving a picture of the somatic characteristics of the people under consideration then it is justifiable to include it along with the other human phenomena, provided we appreciate its limitations.

Racial geography contains many pitfalls for the unwary, but this is no justification for the scant attention it so frequently receives, if only for the fact that such neglect deprives geographers of an opportunity to dispel much of the prejudice existing in the world to-day.

It would be a great gain if the word race could be dropped entirely when speaking of human groups, since any strict application of the term is now impossible—if it ever was possible—owing to the intermixture which has been going on for a very long period. Moreover, much confusion and even mischief have been caused by the misuse of the term in connection with national, linguistic and religious differences which have to be carefully distinguished from genetic differences. Attempts to find a satisfactory alternative term for what is a biologically differentiated group have not been successful ; and after much discussion between leading physical anthropologists and geneticists at meetings sponsored by Unesco, an agreed statement on the nature of race and race differences was issued in September 1952. Booklets containing these conclusions and elaborating many topics arising out of them have since been published by Unesco. Amongst other things it was decided that ' in its anthropological sense the word race should be reserved for groups of mankind possessing well-developed and primarily heritable physical differences from other groups ' (14). Therefore, in describing races of man, reference can be made only to physical characters such as hair texture, skin colour, head form, or blood groups, and it must be stressed that these characters carry no implications regarding mental or cultural qualities since there is no demonstrated connection between them. Furthermore, there is no evidence for the existence of so-called ' pure ' races ; on the contrary all races are mixed, and there are no scientific grounds whatever for dubious doctrines of racial superiority or inferiority. Again, it is quite absurd to speak of an English race, or an American, or a German or a Hungarian race but this error is still habitually committed, even amongst geographers.

It is thus all too clear that much that has been written under the heading of racial geography has to be regarded with extreme caution if not completely abandoned. Especially is this true when the precedence of different types is involved whether this favours the long-headed Nordic of northwest Europe, or the broad-headed Alpine

occupying the heart of Eurasia, where, according to Griffith Taylor's scheme, we are alleged to get ' the latest development of the human race ' (15). Mistakes have, of course, been made by professed anthropologists, and reference may be made here to the work by Dixon in *The Racial History of Man* whose attempt to define a number of primitive pure human races has been condemned by himself as ' a crime ' (16).

In the light of what has been said it may well be questioned whether it is worth while introducing the subject into a geographical study at all ; and the reply must be to repeat that it is still essential if only for describing the various physical characters of human groups, while it is of particular value, on the negative side, in condemning views of racial inequality based on ignorance and prejudice. This follows from one of the conclusions in the Unesco statement already mentioned, and it may be noted that Professor Klineberg affirms that ' every racial group contains individuals who are well endowed, others who are inferior and still others in between.' He then asserts, though no doubt all would not agree, that ' the range of capacities and the frequency of occurrence of various levels of inherited ability are about the same in all racial groups ' (17).

As to the physical characters employed in making racial classifications, genetical examination of traits accepted earlier, such as head form, and so on, has not greatly modified views as to their suitability, and schemes based on these characters continue to be made, such as that by Coon, Garn and Birdsell (18). On the other hand geneticists have added blood groups, principally the O, A, B, AB groups, on which to base a classification, and from some points of view they are superior to the other tests, since not only are blood groups determined solely by heredity but their inheritance is very simple while that of stature, head form and colour is more complex genetically. Moreover, there can be no prejudices against blood group genes since they do not produce any observable feature ; but it must be admitted that the scheme of classification so far worked out by Boyd does not seem to take us beyond six generalised races based on continental areas. A further point is that the distribution of particular blood groups conflicts at times with distributions based on other racial characters, as witness the map by Boyd showing blood group B which reveals the highest frequency in northern India, China and Mongolia, with a secondary high in Central Africa. On the other hand, within Britain, blood group evidence supports the idea of Viking settlement in parts of Pembrokeshire, as it also does the survival of a very early human type around Plynlimmon and other moorlands in Wales, so that it can be of significance in historical geography (19).

Turning to social geography we find a branch which has made rapid progress in recent years. Roxby was conscious of the important role it ought to play in human geography, but he did not attempt any detailed analysis of its content, though he pointed out the danger of overstressing the purely economic aspects of geography. It is not easy to define its scope precisely, but it aims at analysing the essential characteristics of man in society within different areas or regions ; and amongst those who have endeavoured to describe its content may be noted Gilbert and Steel in a survey of the place of social geography in colonial studies (20), also Dr. Wreford Watson who, in addition to surveying the general scope of the subject, has analysed the social geography of the city of Hamilton, Ontario (21).

As regards content, population and its distribution over the earth must come first ; and under this heading are included not only density but also such details of composition as age and sex, and characteristics regarding languages, religions, education and occupations. Many of these data are often referred to as demography. Objection may be taken to the inclusion of such details as sex or age characteristics on the ground that they belong to sociology. But surely these features form an essential aspect of the population of some particular area and they are legitimately part of the study of areal differentiation. Maps showing sex distribution have been prepared, for example, by Dr. Wise for the Cannock Chase area of Staffordshire where there is a notably high proportion of males to females in the

coal mining districts owing to the dominance of that industry. Lack of suitable industries there for young women has caused a definite migration to neighbouring towns, particularly Birmingham, thereby giving a lack of balance to the population and adding to the social problems of the area (22). As to age structure, Dudley Stamp has recently illustrated the geographical significance of this in a survey of some world food problems (23).

Secondly, come the form and distribution of rural settlements ranging from villages to hamlets and isolated farmsteads. This section provides a topic of wide scope not only from the historical point of view, as witness Mr. H. Thorpe's studies of village forms in England and Denmark (24), but also as regards newer forms of settlements, such as the *ejido* of Mexico and the *kvutza* or modern communal village forms in Palestine. Any satisfactory survey of the present human geography of Israel must, surely, include a study of the latter which, in their orderly pattern, stand out in marked contrast to the more inchoate forms characteristic of Arab settlements, but they are all phenomena which are more directly related to the cultural background and ideologies of the two groups rather than to the physical environment. Thirdly, there is the study of towns and cities, or urban geography, which has now produced a considerable amount of literature, including especially the studies made by Professor R. E. Dickinson and Professor Griffith Taylor. Finally, the subject of migration is itself a big field but one of great significance in certain areas such as Africa and the U.S.A. not to mention the cases of Europe and the Middle East where problems of refugees now loom so large.

No better illustrations of the importance of certain aspects of racial and social conditions can be found than in the Americas, where it is essential for geographers to study these if they are to obtain any true appreciation of the personalities of the different areas. From the strictly racial or biological standpoint no other part of the world exhibits such a mixture, containing not only American Indian, of Mongoloid affinity, but also the African Negroid, once a slave, with every conceivable variety of the White European, or Caucasoid type, and finally, representatives of modern types of mixed Mongoloid peoples in the form of Chinese and Japanese. It is within Latin America that the greatest mixture exists, and there, Brazil, which inherited the medieval racial tolerance from Portugal, has no colour problem, so that the Brazilian population probably approximates most closely to what has been termed the ' cosmic race ' (25) although it contains a great diversity of racial and social types. As evidence of the success of miscegenation a recent writer has urged that ' no tropical country has shown such purpose and such cultural originality as Brazil' (26). The populations of Argentina and Uruguay are mainly of mixed Italian-Spanish origin, while those of Peru and Bolivia have still many pure Quichua or Aymara Indians (more than half in fact), and also many mestizos (Indian-Spanish mixtures) with a nominal 10 per cent. of more or less pure European origin forming the main ruling and landowning class. Countries like the latter exhibit very well the fact that so-called ' racial ' descriptions are frequently dependent on social or economic conditions in that a man ' ceases to be an Indian and becomes a " mestizo " by a change of clothes, and ceases to be a " mestizo " and passes to the upper classes or "whites" by the acquisition of land ' (27). Conversely, it is also certainly true that racial differences are most frequently of significance because of their social and economic implications as is only too apparent in the United States in the cases of the Negroes, and other minority groups. It was the startling contrast between the American dream, or the American creed or ideal, and the American reality in the actual treatment of Negroes that gave Gunnar Myrdal, the Swedish sociologist, the title for his famous study of *An American Dilemma* (first published in 1942) (28). That ideal is the historical ideology inherited from the French Enlightenment and embodied in the early documents of American history, and giving to all the right to 'life, liberty, and the pursuit of happiness,' but the existing state of affairs was often something entirely different.

Without attempting to pursue this matter in any detail it is only right we should note that conditions have been undergoing a fundamental change during the past few decades, and Negroes no longer suffer from the same disabilities as formerly. Factors producing this change include not only a modification in attitude on the part of the whites, but also changing economic conditions which have brought many more Negroes from their former centres in the south-east to cities in the north and the west.

Although the areas of relatively dense Negro population have been primarily associated with the south-eastern region of hot, humid summers and mild winters, it cannot be argued that climate was the sole factor concerned, or even the chief factor in some cases ; and the detailed pattern of Negro or Coloured concentration can only be understood with reference to certain crops (cotton, tobacco, rice, indigo) which depended not only on particular climates but also on certain conditions of surface and soil, and furthermore in relation to the institution of slavery (29). The danger of relying on the climatic factor to 'solve' the Negro problem in America can be well illustrated by the fact that even such a keen observer as Lord Bryce was led quite astray by this argument. He believed that an automatic segregation of the Coloured population was being worked out because of climate so that, he wrote, ' it is clear that the Negro centre of population is more and more shifting southward, and that the African is leaving the cooler, higher and drier lands for regions more resembling his ancient seats in the Old World ' (30). Again, Dr. Ellen C. Semple dogmatically asserted in 1911 that ' the catarrhal zone north of the fortieth parallel soon exterminates the Negroes ' (31). Both prophecies have been falsified, and in New York, Harlem alone has 200,000 Negroes, while Cleveland now has over 150,000 representing about 16 per cent. of its total population. Before leaving the United States we might interject the comment that American problems would probably be viewed with a little more understanding by ourselves if we realised more fully not only the immense size of the country and its natural resources, but, equally important, the mixed nature of its population in many areas. In Detroit, for example, in the later part of 1952, an International Institute was opened to cater for the needs of its many nationalities and creeds, and no less than thirty-nine different cultural groups from Europe, Asia and Africa, as well as the Americas, were listed in the procession programme.

Of the economic aspect it is hardly necessary to speak since its general content is well enough known and there is more universal agreement concerning it. It is, of course, of fundamental importance, but I venture to suggest that, to take just one example, in addition to studying the production and distribution of cocoa in the Gold Coast and Nigeria in relation to climatic and other physical conditions, it is of vital significance to know also that the crop is raised there under the conditions of small peasant proprietorship with all its attendant benefits and risks.

In the general field of economic geography I think it is very fitting to pay tribute to the distinguished work by Professor Wilfred Smith, the present holder of the Chair at Liverpool, and a former student of the School, in his book on the *Economic Geography of Great Britain*, and also in other more specialised studies of location. It is also relevant to note here Professor Smith's approach to the field of economic geography. To him it is not just a discussion of the effect of physical factors on economic production, but, like geography in the total sense, it is concerned with the recording and understanding of facts, not only with facts at one particular point of time but with the whole sequence of their development. So again ' economic distributions are shaped by a complex of factors, physical and technical, historical and economic, social and cultural, and it is the economic geographer's task from a review of *all* the evidence to discover which operate in each particular case ' (32).

Political geography, the last of the aspects of human geography, represents a summary of all the other aspects since its function is ' to study and appraise the significance of political and administrative units in relation to all the major geographical groupings, whether physical, ethnographic, social or economic, which

affect mankind.' It is precisely because there are so many aspects of adjustment—and often in conflict with one another—that a satisfactory solution is so difficult to attain, and the political history of the world since the First World War is a sad but eloquent enough witness of this fact. When Roxby wrote, he quoted examples from the political map of Europe with its new nation-states to press home the point that the realities to be considered were not merely those of physical and economic geography, but also included those often rather intangible factors compounded of sentiment and tradition which help to produce what is called nationality. The resulting conditions were hardly ideal, and since then the active intervention of an ideology into the political arena let down an 'iron curtain' which has brought other woes in its train, although it has also imposed some measure of unity on that part of Europe lying to the east of the 'curtain,' and it has indirectly stimulated the growth, to the west of it, of a unified Western Europe which can be immensely beneficial in economic and other ways in days to come. Again, the modern political developments in Nigeria, Britain's largest remaining colony, have been to reverse the earlier policy of centralisation which prevailed during the first quarter of the century in favour of the creation of three separate regions—Northern, Western and Eastern—each with a Regional House of Assembly, while there will also be a central legislature at Lagos to secure administrative unity within the federal area. The formation of these regions is a recognition of the existence of differences, in some cases very acute differences, in human geography which have shown themselves in religion, language and other cultural characteristics in the dominant groups occupying each area, the Hausa-Fulani in the North, the Yorubas in the West, and the Ibos in the East. As might be expected, there remain several problems which necessitate very sympathetic handling, such as the future political status of the so-called Middle Belt, ethnically a shatter zone, now part of the Northern Region but claiming to be a separate unit (33); but despite these difficulties, the steps already taken towards self-government within Nigeria have been of vital significance, and they are to be welcomed as movements in the right direction.

These then are the constituent elements of human geography and when viewed in this way I feel that the term 'human' is still the best general one to use, since not only has the term now become traditional but to the others that have been proposed, particularly 'social' and 'cultural,' there are very definite objections. As will now be evident the first of these is actually used to cover only a part—admittedly a very important part—of the whole, while the word *cultural* is used in a variety of ways. In its more precise interpretation it refers to the socially acquired habits, skills and attitudes of peoples together with their material embodiments in tools, artefacts, buildings and the like, and thus it is closely linked with social geography itself and is grouped with it. In a broader sense, however, the word is regarded as being more or less synonymous with 'human,' or, at least with the social, economic and political aspects of this part of geography, and it is in this sense that the term is used by many American geographers, e.g. R. J. Russell and F. B. Kniffen in their book on *Culture Worlds*.

Roxby's insistence on historical geography was, I think, of paramount significance in contributing to the healthy growth of the subject, and it has been abundantly justified by the results that have ensued. By historical geography is meant surely not the explanation of historical events by physical conditions, or the illustration of history by a few maps, but it is 'essentially human geography in its evolutionary aspects.' As such it is a pre-requisite for any sound regional study, as witness Roxby's own published work on East Anglia and China, while it has proved itself more and more to be an illuminating approach to the understanding of any period of history or prehistory. Evidence of this is readily forthcoming, for example, in the case of Professor Bowen, whose work on the *Travels of the Celtic Saints* (34) has done much to disentangle the early or proto-historic, and later prehistoric periods of Western or 'Highland' Britain; and another striking example of the success achieved in this field is seen in the publication of the *Domesday*

Geography in which Professor Darby and other scholars have contributed something of fundamental value to the study of Domesday England which other workers have failed to produce despite a century or more of commentary on the Domesday text itself (35). As an example of another kind we might refer to the existing localisation of industries in the Birmingham-Black Country Conurbation, where studies by Dr. Wise, Mr. B. L. C. Johnson and others have made it abundantly clear that the present pattern bears little or no relationship to the physical geography of to-day, and can only be understood in the light of the historical context of the time when the pattern was first formulated (36).

Next a word regarding regional geography. Roxby was quite definitely a regional geographer in the sense that he firmly believed that all the different aspects of physical and human geography find their fullest fruition when brought together and inter-related in a comprehensive treatment of some region, whether that be the Balkan peninsula, tropical Africa, or the Far East, or the local area. He forecast, indeed, that geographical specialisation in the universities would tend to become relatively more concerned with regions such as those just enumerated, and less with general or systematic aspects (e.g. geomorphology, economic geography), although the latter would always have a place, as they must in any balanced course.

Another achievement which revealed his initiative and foresight, as well as his great energy, was the group known as the Liverpool and District Regional Survey Association which came into being in 1918. It brought together workers in diverse fields—academic, social, medical, industrial—and it included such well-known figures as Harold Peake, Professor P. G. H. Boswell and Professor (now Sir Patrick) Abercrombie, then Professor of Civic Design at Liverpool, and there can be no doubt that not a little of the valuable contribution that geographers have since made in the sphere of planning can be traced back to the pioneer work carried out in Liverpool. It is significant that both Roxby and Abercrombie were co-opted as members of the South West Lancashire Joint Town Planning Advisory Committee which issued its report in 1930.

On the methodological side he published two papers on natural regions, and the first of these, in 1907, is especially interesting in giving some indication of the distance he had covered in thought between his earlier and his more mature years. In that article Roxby enthusiastically pursued the theme opened up by Herbertson in his classical paper of 1905, and declared quite boldly that '*ceteris paribus*, a particular set of physical conditions will lead to a particular type of economic life. A physical unit tends to become an economic unit' (37). This hardly sounds like the Roxby who was later to write such illuminating articles on Chinese life and culture! Even in 1907 he appreciated the existence of other vital factors such as the system of land tenure, and the character of the peoples in the area, that modified the normal economic working of particular physical circumstances; moreover, he safeguarded himself by the phrase '*ceteris paribus*' since, in geography, things are never equal, for, as Mackinder once expressed it, 'there is no repetition of the pattern; geography no more repeats itself than history.' On the other hand, so deeply ingrained had the idea of the natural region become that, even in 1926, Roxby persisted in using the term 'natural' for such an entity as Central Europe, despite the significance of cultural and other factors in its evolution, on the ground that 'one ought to think of Man as part of Nature in regional geography' (38). The whole question of regions and their definition is, of course, notoriously one on which much geographical ink has been spilt, and I must resist the temptation to pursue the matter further at this stage, except to commend to you a recent summary by Preston E. James which neatly clarifies the present situation. 'There can,' he writes, 'be no such thing as a correct system of regions, or a system of "true regions"; no one system of regions is right and all others wrong: there are as many regional systems as there are problems worth studying' (39).

In the little time that remains I will refer briefly to just three points.

Firstly, I can well imagine objection being raised to extending the range of the geographer's contacts with the various social or human sciences on the plea that he already has quite enough to cope with. The same kind of argument was, in truth, urged in the earlier years of this century against the proposed extension of the geographer's interest into the realm of the natural sciences, and it makes strange reading to turn again to the discussion at the Royal Geographical Society following Herbertson's famous paper on Natural Regions in 1905. On that occasion Dr. H. R. Mill expressed the view that the geographer did not 'require to go beyond the purely orographical' in his explanations, and Douglas Freshfield followed up with the lament that 'as we grow more and more scientific we are liable to mix up geography with every other science on the face of the Earth.' He especially implored his audience to 'keep geography as far as possible to a definite field and not to intrude on geology or other sciences' (40). Geography would certainly have wilted if this advice had been followed, and I am convinced that it will only flourish as it should when its growth is stimulated by the application not only of the natural but also of the human elements.

Secondly, this leads to the question as to the nature of 'geographical facts,' and I have on more than one occasion argued against the practice of identifying these only with the facts of natural phenomena in addition to the facts of location. We are, I feel, compelled to accept the view that all facts are of geographical significance in so far as they contribute to the understanding of the areal differentiation of the world (41). This point of view is now held by an increasing number of British geographers, and I discovered on a recent visit to the United States that it has gained wide acceptance also among leading American geographers.

Finally, a word about Roxby's interest in education. It is true he deliberately chose to be a teacher rather than a writer, though this should not make us lose sight of the fact that he published quite a number of articles, principally, of course, on China. To him the primary objective of geographical education was the development of citizenship in the fullest sense both at home and in the world at large (42). Human geography, rightly handled, was the subject *par excellence* to provide the fundamental background for the understanding of many of the problems besetting mankind, in the local, national or international fields. The world is indeed the geographer's parish, and it is fitting that we conclude by repeating the time-honoured maxim to every geographer that he should learn something about all of it, and all he possibly can about some of it.

References

(1) Roxby, P. M. (1930). *The Scope and Aims of Human Geography* (British Association, Bristol, Presidential Address to Section E), p. 5. *Scot. Geog. Mag.* **46**, 1930, pp. 276–290.

(2) Roxby, P. M. (1914). *Report to the Trustees*, Albert Kahn Travelling Fellowships, pp. 41–74. London.

(3) *Geographical Journal* (1951), **117**, p. 458; the other lists of definitions so far are in **118** (1952), pp. 69, 209, 345.

(4) Hartshorne, R. (1939). 'The Nature of Geography,' *Annals of the Association of American Geographers*, **29** (1939), p. 101.

(5) Wooldridge, S. W. (1945). *The Geographer as Scientist*, Inaugural Lecture, p. 17. London.

(6) Stannard, H. (1945). *What is a Nation?* Looking Forward Pamphlets, Royal Institute of International Affairs, pp. 52–53.

(7) Wooldridge, S. W. & East, W. G. (1951). *The Spirit and Purpose of Geography*, p. 28. London.

(8) Edwards, K. C. (1950). *Land Area and Region*, Inaugural Lecture, University of Nottingham, p. 7.

(9) Sauer, C. O. (1952). *Agricultural Origins and Dispersals*, Bowman Memorial Lectures, *Amer. Geog. Soc.*, pp. 2–3. New York.

(10) James, Preston E. & Kline, Hibberd V. B. (1949). *A Geography of Man*, p. 165. Boston.

(11) Herbertson, A. J. (1915). 'Regional Environment, Heredity and Consciousness,' *Geographical Teacher*, **8**, p. 3.

(12) *Geographical Journal* (1951), **117**, p. 481.

(13) Hartshorne, R., *op. cit.*, p. 147.

(14) *What is Race?* Evidence from Scientists, Unesco (1952), p. 83. Other publications in this series include, M. Leiris, *Race and Culture*; G. M. Morant, *The Significance of Racial Differences*; Otto Klineberg, *Race and Psychology*; Arnold M. Rose, *The Roots of Prejudice*.

(15) Taylor, Griffith (1951). 'Racial Geography,' in *Geography in the Twentieth Century*, p. 459. New York and London.

(16) Boyd, W. C. (1950). *Genetics and the Races of Man*, p. 196. Oxford.

(17) Klineberg, Otto (1951). *Race and Psychology*, Unesco, p. 39.

(18) Coon, C. S., Garn, S. M. & Birdsell, J. B. (1950). *Races . . . A Study of the Problems of Race Formation in Man.* Oxford.

(19) Fraser Roberts, Dr. J. A. (1949). 'Blood Groups and Human Genetics, in Human Blood Groups,' *Advancement of Science*, **5**, No. 20, pp. 305–307; also, 'History in your Blood,' *Eugenics Review* (1952), **44**, pp. 28–30; Boyd's map showing isogenes for blood group genes A and B are printed in his *Genetics and the Races of Man*, pp. 228–229.

(20) *Geographical Journal* (1945), **106**, pp. 118–131.

(21) Watson, J. Wreford (1951). 'The Sociological Aspects of Geography,' in *Geography in the Twentieth Century*, ed. Griffith Taylor, pp. 463–499. New York and London.

(22) Wise, M. J. (1951). 'Growth of Population in the Cannock Chase Coalfield,' *Geography*, **36**, pp. 235–248.

(23) Stamp, L. Dudley (1952). *Our Underdeveloped World*, pp. 32–44. London.

(24) Thorpe, H. (1951). *Institute of British Geographers*, **15**, pp. 153–180, and **17** (1952), pp. 113–129.

(25) *The Republics of South America* (1937), R.I.I.A., p. 63. London.

(26) Camacho, J. A. (1952). *Brazil, An Interim Assessment*, pp. 23–26, R.I.I.A., London; also, Butland, G. J. (1951). *Chile*, R.I.I.A., pp. 13 ff. London.

(27) *The Republics of South America* (1937), R.I.I.A., p. 60. London.

(28) Schermerhorn, R. A. (1949). *These Our People, Minorities in American Culture*, pp. 13 ff. Boston.

(29) Hartshorne, R. (1938). 'Racial Maps of the United States,' *Geographical Review*, **28**, pp. 277–278.

(30) Bryce, J. (1923). *The American Commonwealth*, New York, new edition, pp. 512–513; quoted by Donald Young; *American Minority Peoples*, 1932, pp. 56–57. New York.

(31) Semple, Ellen C. (1911). *Influences of Geographic Environment*, p. 37. New York.

(32) Smith, Wilfred (1952). *Geography and the Location of Industry*, an Inaugural Lecture, University of Liverpool, pp. 3–5.

(33) Buchanan, K. (1952). 'Nigeria, Largest Remaining British Colony,' *Economic Geography*, **28**, pp. 302–322.

(34) Bowen, E. G. (1944). *Antiquity*, **18**, pp. 16–28.

(35) Darby, H. C. (ed.). *The Domesday Geography of England*, Cambridge (to be completed in 6 volumes; Vol. I was published in 1953); R. H. Kinvig, 'The Birmingham District in Domesday Times,' in *Birmingham and its Regional Setting*, 1950, pp. 113–134. Birmingham.

(36) Wise, M. J. & Johnson, B. L. C. (1950). 'The Changing Regional Pattern during the Eighteenth Century,' and 'The Black Country 1800–1950,' in *Birmingham and its Regional Setting*, pp. 161–186; 229–248. Birmingham.

(37) Roxby, P. M. (1907). 'What is a Natural Region?' *Geographical Teacher*, **4**, pp. 123–128.

(38) Roxby, P. M. (1926). 'The Theory of Natural Regions,' *Geographical Teacher*, **13**, pp. 376–382.

(39) James, Preston E. (1952). 'Towards a Further Understanding of the Regional Concept,' *Annals of the Association of American Geographers*, **42**, p. 200.

(40) *Geographical Journal*, **25** (1905), pp. 300–312.

(41) Hartshorne, R., *Nature of Geography*, pp. 372 ff.

(42) 'Geography and Citizenship' in *Geography in Education* (Geographical Association), (1919), pp. 37–40.

OPEN AND REPRESSED MONETARY INFLATION

Address by

PROF. F. W. PAISH

PRESIDENT OF SECTION F

INTRODUCTION

ONE of the first essays I was set to write as an undergraduate at Cambridge was on whether economics is a science or an art. I do not think I can have written a very good essay, for I am sure that I had then not the slightest idea of what the question was really about. But the older I get the surer I become that economics, or at least applied economics, is much more an art than a science. The element of personal idiosyncrasy resides not so much in the effort to discern causal relationships between given phenomena, for much of this is a branch of logic or even of mathematics, as in the selection of phenomena which we choose to regard as significant. In the world to-day there are over two thousand million persons, each with many sets of different relationships with other persons. Out of this enormous welter of many thousands of millions of partially related variables, with both the variables themselves and their relationships changing continuously, the applied economist has to select two or three (or if he is a mathematician perhaps six or even a dozen), between which he has to assume relationships which are reasonably stable; and he has to select these variables in such a way that, for the particular period and purpose that he has in mind, he can ignore the effects of all the millions of others and draw practical conclusions only from the few he has selected. Nor can he assume that the same choice of factors will remain relevant for long, even for the same type of problem. Old factors lose part of their relevance and new ones grow in significance, so that, even if by some miracle we are originally right in our choice of factors, we soon begin to drift into error unless we continuously revise our choice of relevant data.

Is it then to be wondered at that economists are notoriously better able to foretell the past than the future? And is it any wonder that a dozen different economists, surveying the seething, shimmering, continually changing complex of factors which constitutes our world, should each select a different combination of factors that seem to them of prime significance, just as a dozen different artists may see a dozen different significant patterns in the same landscape.

The highly simplified model which every applied economist, like every other human being, constructs for himself as an aid to the understanding of an infinitely complex real world, is therefore an inevitably subjective creation, often inappropriate to others even for the limited purposes for which it was designed. Speaking for myself, I often think I find it easier to understand the real world, infinitely complicated though it is, than the simplifications of some of my colleagues whose models are not the same as mine, just as, say, Sir Alfred Munnings seems to find it difficult to understand the simplifications of Picasso, and perhaps vice versa.

Thus in putting before you to-day my own simplified model, devised for the particular purpose of examining problems of monetary inflation in Britain during the recent past, I cannot expect that everyone, or be sure that anyone, will find it useful. All that I can claim is that this is a way that I myself have found a useful one of looking at the world, and I am not altogether without hope that there may be some of my listeners who may find it useful too.

What do we mean when we use the word 'inflation'? For some people it is merely a condition in which prices rise rapidly for a considerable time; but this definition is not only too wide but is also couched in terms of symptoms rather than of causes. For me, inflation is essentially a monetary phenomenon, whereas a rise in prices can be caused by a fall in physical output as well as by changes originating on the monetary side. Nor for me is a rise in prices the essential characteristic of an inflation, although it is a normal symptom of it. I prefer to relate the phenomenon of an inflation to a rise in money incomes rather than in prices, and to define it in the first place as a condition in which money incomes, almost entirely personal money incomes, show a persistent increase. This is still, however, not a sufficient definition, for a rise in personal money incomes, accompanied by a corresponding or faster increase in the quantity of things to buy, so that prices do not rise, is not ordinarily called an inflation. We must therefore add to our definition that personal money incomes must not only be rising, but must be rising faster than personal real incomes.

While this definition is formally satisfactory, not all the conditions to which it might apply would be regarded as undesirable. Even in the depths of a trade depression, with many factors of production unemployed, a recovery in output will very possibly be accompanied by a somewhat more rapid rise in money incomes and by some rise in prices; but a rise of, say, 50 per cent. in money incomes accompanied by a rise of 49 per cent. in real incomes would be regarded by most people as a desirable step towards recovery, and would probably be classified, to use the jargon of the recent past, as 'reflation' rather than 'inflation.' At the other extreme, even in an active boom with very few unemployed factors, a further rise in money incomes might very well (though by no means necessarily) be accompanied by some slight rise in real incomes; but in such conditions a rise of 50 per cent. in money incomes would undoubtedly be regarded as undesirably inflationary even if it were accompanied by, say, a 1 per cent. rise in real incomes. How large a rise in real incomes would have to accompany a given rise in money incomes to make the inflation desirable is necessarily a subjective judgment, depending often on the personal circumstances of the observer; but in general it can be said that, especially when there are few unemployed factors, the faster money incomes rise the less likely will it be that real incomes will rise sufficiently to compensate for the disadvantages of inflation in the destruction of money savings, the arbitrary redistribution of wealth and incomes, and the distortion of the economic and often the corruption of the political system.

While for me the essential element of an inflation is a rise in personal money incomes, its essential cause is that collectively the community overspends its income. If we like, we can divide expenditure into two rather arbitrarily defined parts, and call one consumption and the other investment. We can also, if we like, divide income into two parts, one part allocated to expenditure on consumption and the other called saving. Since the part of income allocated to consumption is by definition the same as the amount spent on consumption, any excess of expenditure over income is equal to the excess of investment over saving. Under present conditions, however, I am not sure that the alternative way of looking at the matter is not equally valid. Often to-day the first charge on income is not consumption expenditure but contractual saving. Many income receivers first set aside the money for the insurance, for the contribution to the pension fund, for the repayment of the mortgage to the building society and for the hire-purchase payments, and only after they have made provision for these can they devote the rest to current consumption. If a man then overspends his income, is it not at least as true to say that he is spending on consumption more than he has left after providing for investment as that he is investing more than he saves? Perhaps the reason that we look at the phenomenon from the second of these points of view rather than from the first is that we have a familiar word for the margin of income left after providing for current consumption—saving—while we have no such word for the margin left after providing for current investment.

In any case, for my purposes to-day it will be more convenient to look at the relationship between total income and expenditure rather than that between part of income and part of expenditure. But to look effectively at the relationship between income and expenditure we shall have to extend further the range of transactions we have to take into consideration. The expenditure we meet out of income is not gross but net expenditure—total expenditure minus all receipts which are not regarded as income. For my present purpose I want to take account not only of payments made out of income but of all payments, however financed; I can therefore modify my definition of the cause of inflation from an excess of net expenditure over income to an excess of total money payments over total money receipts.

This definition, of course, at once brings us up against the difficulty that, since every payment immediately results in a corresponding receipt, receipts and payments are always equal. I think, however, that we can overcome this conceptual difficulty if we think of receipts as occurring, not quite simultaneously with payments, but an instant later. We can then get a lack of identity between payments and *previous* receipts; for while every payment automatically creates a receipt, every receipt does not create a payment. Clearly this lack of identity between payments and previous receipts must relate to some fairly short period of time; the information that payments had been greater than previous receipts over the whole of the last fifty years would give us no guidance about the way money incomes were changing at the present moment. For this reason I have divided time up into a series of short periods of, say, one week or one month each. The first monetary transactions of each period consist of the receipt of the personal incomes which have been paid at the end of the previous period; the last transactions of each period are the payments of the amounts which will constitute the personal incomes of the next period. (For the purpose of this discussion, personal incomes are defined as all receipts of money by persons which the recipients consider they can spend on current consumption, however defined, without consuming capital).

For all other transactions, both receipts and payments fall within the same income period. (This enables us, if we want to, to net out all non-income transactions, and makes the difference between expenditure and income the same as the difference between total payments and total previous receipts within any income period). These other transactions include all payments to persons other than those which are regarded as income by the recipients, and all payments of every kind to businesses and governments. There is a little difficulty in the treatment of payments to proprietors of sole businesses: we must think of these as separating their personal accounts from those of the business, and paying themselves salaries out of their receipts only at the end of each income period.

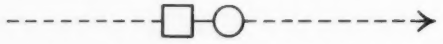
Now let me try to describe my own particular picture of the monetary system. Most pictures of this sort represent as a circular flow a limited type of monetary transactions—those relating to the national income—over a given period, usually a year. They thus omit not only the whole range of non-income transactions, but are unable to indicate changes through time in the volume and direction of the various flows; and it just these changes which for our present purpose we are most interested in watching. My own approach is rather different. Conceptually, I follow the life history of each separate unit of money, observing the circumstances of its creation and disappearance, as well as of its changes of ownership as it passes from bank account to bank account or from pocket to pocket, and even keeping an eye on it when it lies dormant for a period in the same ownership. It is quite possible thus mentally to trace the life history of a unit of money without recourse to visual aids; but I think that my exposition may be made easier with the help of diagrams, and I have therefore constructed a visual model.

In this model, every unit of money traces for itself a separate line through time, rather as if it were making a recording on a barograph. Ideally, the units should be pounds, or even pence; but as a diagram containing even 6,000 million lines would be a little awkward to handle, we must content ourselves with

much larger units—say, of £100 Mn. or so—and even with these we shall have room for only a fraction of the total.

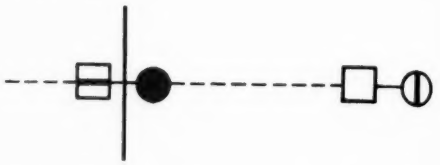
So long as money is held in the same ownership, it is represented by a dotted horizontal line, running from left to right through time :

Every now and then a given unit of money changes hands. The change of ownership will involve a payment by one party and a receipt by the other ; let us show payments by squares and receipts by circles. As every payment necessarily involves a receipt, we will link payments and receipts together by solid lines :



We must now proceed to distinguish the different types of accounts into and out of which payments are made. The simplest model which will be useful for our purpose must distinguish five different types of accounts : personal income accounts ; personal capital accounts ; business accounts ; Government accounts ; and accounts *of* (not of course *with*) banks. These distinctions could, and perhaps for some purposes should, be multiplied almost without limit. For some purposes, for instance, it might be convenient to distinguish payments to retailers from payments to wholesalers or manufacturers ; for others, payments to makers of consumption goods from payments to makers of capital goods ; and for others again, payments for long-term fixed interest securities from payments for short-term securities, or for shares, or for various types of physical assets. But for our purpose to-day these five categories will suffice. Below is our notation.

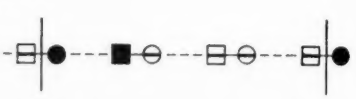
While a payment is automatically and immediately linked to a receipt, the delay between a receipt and a payment may be of indefinite length. When, however, a payment is ultimately made, it must be made out of the same account as that which earlier received the money. The only change in notation between receipt and payment occurs when a person saves part of his income or dissaves part of his capital, and thus converts an income balance into a balance on capital account, or vice versa.



E.g. persons receive wages from businesses, save them and lend them to the Government.

Now let us trace a simple series of monetary transfers through a single income period :

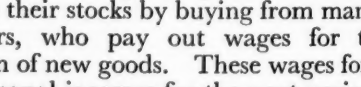
1



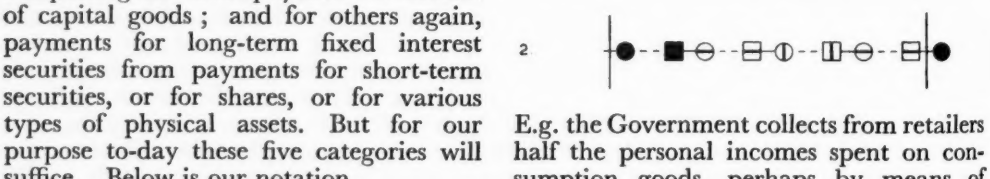
An interpretation of this might be: Persons receive incomes from businesses at the beginning of a period, and spend them on consumption goods. Retailers replace their stocks by buying from manufacturers, who pay out wages for the creation of new goods. These wages form the personal incomes for the next period.

Now let us introduce the Government :

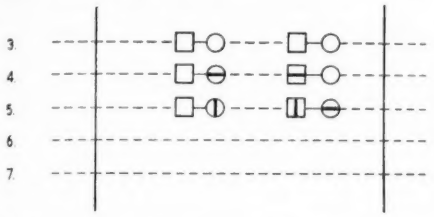
2



E.g. the Government collects from retailers half the personal incomes spent on consumption goods, perhaps by means of purchase taxes, and uses the money to buy goods from manufacturers who pay wages in order to make more.



Meanwhile, other units of money are lying idle, or are being used for the purchase and sale of assets and titles, or for loans and repayments :



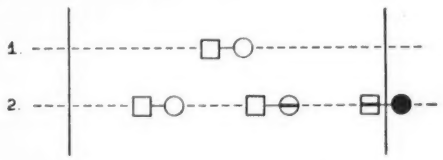
In line 3 a person buys assets, say securities, from another person, who buys more from a third, who holds the money over the end of the period ; in line 4, a person buys securities from a business, which buys others from a person ; and in line 5 a person lends money to the Government which uses it to buy an asset from a business. Lines 6 and 7 are idle balances.

The total quantity of money is found by counting the number of lines (in this case 7), the total turnover of money is found by counting the number of transactions (in this case 13), while the total of personal incomes is two.

Open Inflation

Let us now turn, with the aid of this model, to an examination of the phenomena connected with a changing level of money incomes. On the definitions given here, the rise in the total of money incomes from one income period to the next is identical with the amount by which payments for the period exceed receipts for the same period. There are two ways in which a man can pay out during any period more than he has received in the same period. One is by spending a stock of money which he has carried over from a previous period. This expenditure will, of course, not necessarily generate increased incomes ; as we have seen, if he uses his money for a capital transaction, and if the recipient either holds the money or uses it for another capital transaction, and so on, the money will eventually be carried over to the next period without giving rise to increased incomes. All receipts other than those by persons for the services of themselves or of their assets can be regarded as capital receipts, for they involve merely a change in assets. Only if some seller of an asset pays someone (possibly including himself) for services rendered either in selling the asset or in replacing it does he create money incomes from the following period. It is, of course, perfectly possible that the use of previously idle money balances for the purchase of assets may raise prices of assets ; but until more money is used for buying from persons the *services* of persons or of their assets, in contrast with the assets themselves, the result is merely to raise the price of assets without raising the price of services, or, in other words, to lower the rate of interest.

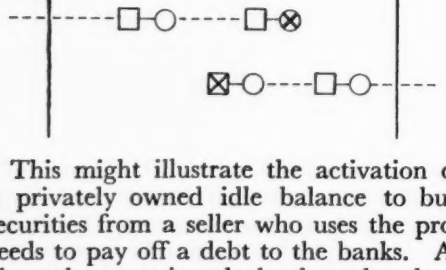
Sooner or later, of course, the rise in prices of existing assets will begin to induce an increase in the demand for new ones, thus increasing the amount of money paid for the services employed in creating them. In times of acute depression, however, when prices of existing assets, and especially of durable assets, may stand well below their replacement cost, there may be a considerable delay before the increase in demand for existing durable assets is reflected in increased orders for new ones, or before holders of excessive stocks are induced by their decrease to increase their replacement orders. In times of active trade, on the other hand, when prices of existing assets are already fully up to parity with those of new ones, a rise in expenditure on buying assets is more likely to be reflected rapidly in a rise in payments for new assets and in incomes :



Of the two instances of the activation of personal idle balances shown above, the first results merely in a change in the ownership of an asset, say a house, while in the second the seller replaces the house by buying from a speculative builder who pays out the money in incomes for building new ones.

A second way in which it might be possible for a person to pay out more money than he had received during the same period would be to make it. As a general practice, the manufacture of money is rightly frowned on by the

authorities. There are, however, certain institutions in a modern system which have this privilege. Exactly what these institutions are varies from country to country, but everywhere the power of creating additional money lies with some combination of the commercial banks, the central bank and the Government. For convenience, we shall call these collectively 'the banks,' and we shall define banks as those institutions whose debts are widely and generally used as money. The banks can, therefore, create money simply by getting into debt, which they normally do by adding to their assets, usually by making a loan or buying securities. Conversely, whenever they reduce their liabilities, usually as the result of selling securities or having loans repaid, they cancel some of the money already in existence. In our model, we therefore show no connection between the receipt of money by the banks and its re-issue ; the money is destroyed by its receipt and created afresh when it is re-issued :

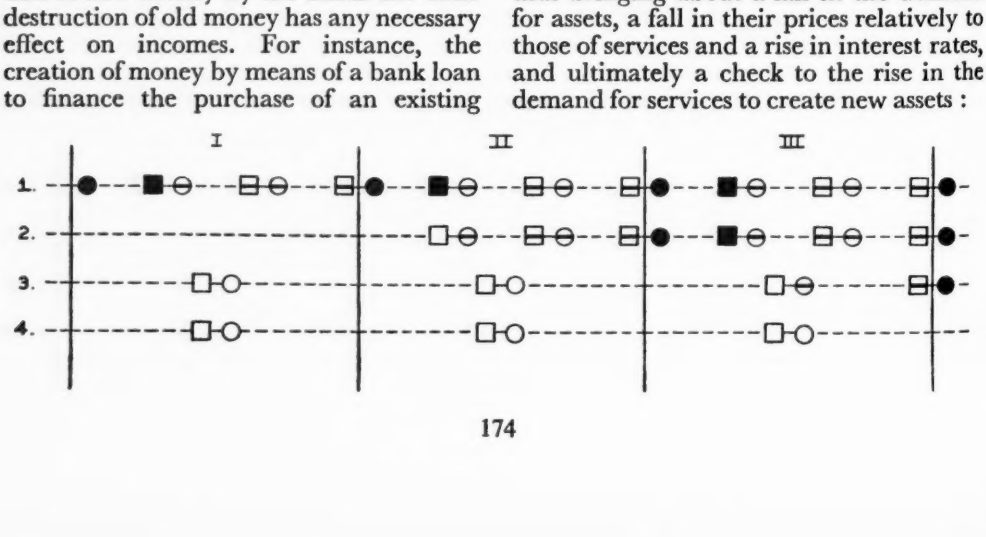


This might illustrate the activation of a privately owned idle balance to buy securities from a seller who uses the proceeds to pay off a debt to the banks. At about the same time the banks make a loan to someone else to buy another security from a private seller.

It should be noted that neither the creation of new money by the banks nor their destruction of old money has any necessary effect on incomes. For instance, the creation of money by means of a bank loan to finance the purchase of an existing house has no immediate effect on incomes, except perhaps of house-agents. Only if the borrower, or one of a series of sellers of existing houses, uses the money to pay for services rendered in building a new one will the rise in the quantity of money be reflected in a rise in incomes :

Of these two instances, the first represents a bank loan spent on buying an existing house from a private owner. In the second, the borrower buys a house from a speculative builder, who uses the proceeds to finance the building of others.

Now let us begin to use our model to look at the genesis and development of an inflationary movement. If idle balances are large (and in practice they are probably many times larger than those being used in income transactions), it would be quite possible for a very considerable rise in money incomes to take place without any increase in the total quantity of money in existence. Sooner or later, however (and probably sooner unless something had occurred greatly to alter the money-using habits of the population) the rising level of incomes, of activity in the markets for assets, and probably of asset prices would absorb the quantity of idle balances that their owners were willing to activate. Further rises in incomes could then occur only by diverting money from the capital circulation, thus bringing about a fall in the demand for assets, a fall in their prices relatively to those of services and a rise in interest rates, and ultimately a check to the rise in the demand for services to create new assets :

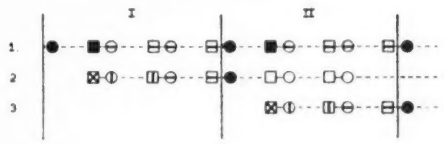


In the above illustration, the total number of units of money is four throughout. In the first period, one of these is used to pay incomes, one is idle, and two are in use for asset transactions. In the second period, the idle balance is activated and used for an income payment, thus doubling the level of incomes. In the third period, incomes can be raised further only by diverting one unit of money from the market for assets, thus leaving, in period four, only one unit in the asset market instead of two, reducing the demand for assets and raising the rate of interest.

It should be noted that if the idle balance had first been activated into the capital circulation, the initial effect would have been to raise prices of assets relatively to those of services and lower the rate of interest. In that case, the rate of interest would not have risen above its original level until not only this unit but another as well had been diverted from the capital circulation to the income circulation.

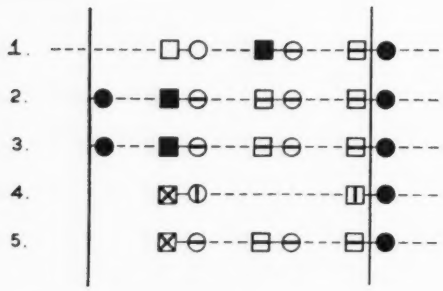
While the increase in incomes induced by an activation of idle balances, though possibly large, is at any rate finite, the increase caused by an increase in the quantity of money is potentially infinite. Like the activation of idle balances, an increase in the quantity of money is not inflationary until it begins to augment personal incomes. So long as its effects are confined to the demand for existing assets, it merely lowers the rate of interest. The speed with which a rise in the quantity of money will be reflected in incomes will depend partly on the way the extra money is created and partly on the economic climate of the period. If, for instance, money is created (as in 1932) by the banks' purchases of securities on the market, or by Government borrowing from the banks to finance the repayment of securities or the purchase of existing assets, the effects may well be confined for a considerable time to the capital circulation and the rate of interest, especially when prices of existing goods stand below the cost of creating new ones.

If, on the other hand, the additional money is lent to businesses to finance new production, or to the Government to pay for the creation of new goods or for services, the effect on incomes will be immediate. Even so, however, the rise in incomes per income period in the early stages of an inflation is likely to be less rapid than the increase in the quantity of money, for the recipients of the increased incomes are likely to save a part of them and so transfer money back to the capital circulation. This is particularly likely to happen when the rise in money incomes is accompanied by a substantial rise in real incomes ; but it may well happen for a time when real incomes are not rising or are even falling. In a country which has not experienced a major inflation within living memory, income receivers may for a time be deterred from buying goods, especially durable goods, by the belief that they are unduly 'expensive,' and that in due course prices will return to a more 'normal' level. The movements of money in the early stages of an inflation may therefore look something like this :



In this illustration, the Government has caused the banks to create for its use an extra unit of money in each of periods I and II, but one of the new units has been diverted to the capital circulation, so that incomes per period have risen by only one unit. If the unit diverted had been used to buy new Government securities instead of an existing asset, the Government would not have needed to borrow at all from the banks in Period II.

This early stage of an inflation is a temporary one. If the inflation is at all rapid, as it proceeds the expectation that prices will rise further will become increasingly general, and income receivers will not only cease to save but will begin to activate idle balances in order to accelerate their purchases, especially of durable consumption goods; at the same time both income receivers and businesses will endeavour to supplement their existing balances by borrowing from the banks. Incomes will then begin to rise faster than the quantity of money and faster still than Government borrowings from the banks (see diagram on page 176).



In this illustration, Government borrowings from the banks during the period have risen by one unit, but the total quantity of money has risen by two units, and, thanks to the activation of idle balances, incomes by three units.

As the existence of the inflation becomes more and more obvious to more and more people, so do the activation of idle balances increase and money incomes rise progressively faster than the quantity of money. In the last stages of the inflation the whole model collapses, for the income period itself is shortened and wages are paid more and more frequently, first, perhaps, twice weekly, then daily, and then twice a day, while income receivers rush to spend their money the moment they receive it. When this happens, however much new money is created, its aggregate value falls faster and faster towards zero until it disappears altogether. At the last, the use of the existing money is abandoned, and until a new money which commands confidence can be introduced, the economy reverts either to barter or to the use of some commodity as a substitute for money.

Repressed Inflation

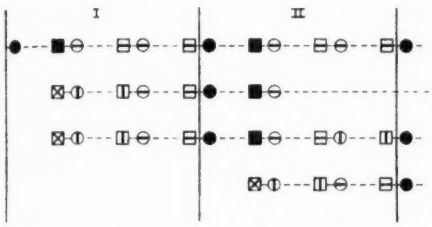
Whereas the First World War provided some classic examples of open inflation, it is to the second that we must look for fully developed examples of repressed inflations, which in the first were only embryonic. The essential function of a Government-induced inflation, such as occurs during a war, is to transfer a larger share of the resources of the country from the service of the private sector to the service of the Government. In an open inflation, this function is performed most satisfactorily in the early stages, when personal spending rises less rapidly than personal incomes and personal incomes less rapidly than the quantity of money; and progressively less satisfactorily in the later stages, when personal spending rises faster than incomes and incomes faster than money. The task of a government, in repressing an inflation, is therefore to prolong indefinitely the initial phase and to prevent the later phase from developing at all.

The method which usually seems to be the first to occur to governments is the fixing of maximum prices. This is perhaps partly because the Government realises that higher prices stimulate demands for higher wages, and therefore may lead either to strikes or to the activation of business balances or business borrowing from the banks in order to pay them. But it is perhaps also partly because governments tend to treat symptoms rather than causes, and rising prices are the most obvious and unpopular symptom of an inflation.

The fixing of maximum prices of consumption goods, at a time when the Government is raising prices of services and incomes by bidding factors away from the service of the private sector with newly created money, tends to have a double effect: to reduce the quantity produced and to increase the quantity consumed. In point of fact, however, the British Government never seems to have relied for reducing the quantity of goods and services supplied to the private sector on making their production unprofitable. It seems to have preferred to restrict output by restricting supplies of materials and labour, or by direct limitation of the amounts or types of goods allowed to be produced. For such output as was still permitted prices seem to have been fixed at a level which made production of that amount quite adequately profitable, even though these prices were still well below those which would have equated demand with the reduced supply.

On the side of demand, the fixing of maximum prices slows down the rise in personal incomes in three different ways. In the first place, by reducing the pressure for higher wages under the threat of strikes, it reduces the tendency for the money created for the use of the Government to be supplemented by money activated or created for the use of

businesses. Secondly, it makes it impossible for retailers and wholesalers to protect their stocks by raising prices, while their full replenishment is prevented by direct controls. Thus in the early stages of price control the increased demands of consumers can be satisfied for a time out of depletion of stocks. During this period, part of the money paid by consumers remains immobilised in the hands of retailers and wholesalers, to be held idle or lent to the Government instead of being spent on the creation of new goods and of personal incomes :

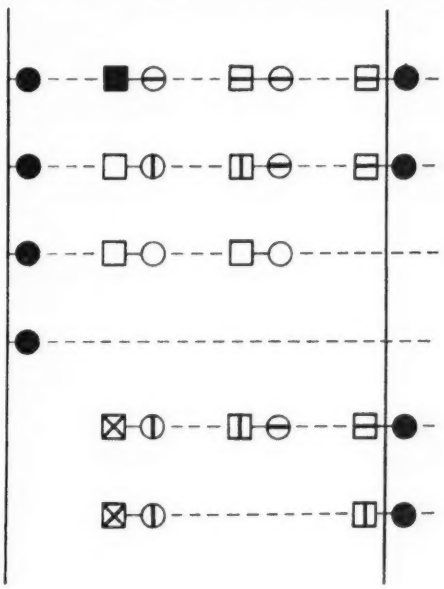


This stage of the repression of an inflation by price controls is of limited duration. Once stocks are fully depleted, consumers begin to be unable to find means of spending the whole of their incomes, at least on things which they prefer to the possibility of spending savings at some future date on things they want more. When this stage is reached, the excess incomes begin to accumulate in the hands of persons rather than of businesses. If these lend them to the Government, the diversion of the control over resources is achieved directly. If they hold them idle or pass them round in the capital circulation, the Government can cause the banks to create more money to take their place without raising incomes (see diagram in next column).

Of these four units of personal income, three are diverted to Government use. One is lent to the Government by persons, one is diverted to the personal capital circulation, no doubt raising prices and reducing interest rates there, and one is held idle. The two last units are replaced by new money borrowed from the banks. Personal incomes remain unchanged.

The supplementation of price control by a system of rationing makes no fundamental difference to this process, for though it distributes consumption goods differently between consumers it does not change the aggregate amount that can be spent on them. It does, however, make the system of controls more politically acceptable, greatly reduces the scope for favouritism in the distribution of scarce supplies, and probably also reduces the incentive to break the law. Adjusted by rationing to appear at least superficially equitable, and supplemented by controls over capital construction and the distribution of producers' goods, these methods of price control appear to operate as reasonably successful repressers of inflation so long as a war actually continues.

One possible method of checking demand, which seems to have considerable merit in principle, but which never seems to have been tried in practice, is rationing *without* price control. If, for instance, every consumer were given coupons to the face value of, say £1 per week which, together with money to the same value, would have to be surrendered in payment for certain classes of goods, the maximum total weekly demand for these classes of goods would be limited to as many pounds as there were consumers. If demand for one of the classes of goods rose, that for the others would necessarily fall ; or if demand for all classes rose in one part of the country, it would necessarily fall elsewhere. If prices of these goods were then allowed to rise and fall freely in accordance with changes in demand, the variations



in prices between products or between areas would greatly facilitate adjustments in the distribution both of resources between different products and of products between different areas.

Whatever the precise method of its implementation, and however successful it is in restraining inflation during the war, a policy of limiting expenditure by means of direct controls leaves a very difficult situation when the war comes to an end. Since both consumers and businesses have been prevented from spending as large a part of their monetary receipts as they would have wished, they have been obliged to accumulate either idle balances or Government securities in excess of their desires. At the same time, much of the expenditure prevented in wartime, especially on the replacement or repair of assets, is merely postponed. The difficulty of the situation is still further increased if, as in Britain, many of the Government securities issued and obligations incurred during the war are repayable on demand or short notice, so that their owners have the power at any time to force the Government to create additional money in order to meet its obligations to repay. Thus, even though the Government itself rapidly ceases to rely on the creation of new money for the finance of a deficit there are present all the potentialities of a renewed and accelerated inflation through the activation of excess idle balances or the enforced creation of new money to make repayments of Government debt.

In face of this potential inflation, the Government has the choice of a number of possible policies, alone or in combination. One of these is to continue the methods of direct control of demand that have been in force during the war. Unfortunately, such controls seem to operate much less satisfactorily in peacetime than in time of war. As soon as the stimulus of the common danger and the single objective is removed, income earners and businesses become progressively less willing to produce and trade in order to obtain money which they cannot spend on what they want to buy; thus production is checked, and more of what is produced tends either to be consumed by its producers or bartered directly for other goods. At the last, if rigid controls are enforced indefinitely, the use of legal tender money may be abandoned entirely over large sectors of the economy in favour of barter or the use of some kind of commodity money; and the final stage of a repressed inflation may thus be not dissimilar to that of an open inflation. Even if the policy of control is not carried to the point of this ultimate breakdown, the effects of partial controls are in their own way almost equally unsatisfactory. If, for instance, as is often the case, controls are more effectively enforced over demand for essentials than for non-essentials, the resources released by the fall in defence expenditure tend to be absorbed in supplying less essential, rather than more essential needs, whether at home or for exports. Further, with the loss of wartime incentives and the release of much enterprise and initiative from the armed forces, the control laws become increasingly difficult to enforce and breaches of them increasingly frequent. Unless the Government is prepared to use more resources for enforcement than it can well afford, and more drastic methods of enforcement than public opinion is likely to tolerate, the continuation of wartime physical controls over expenditure therefore cannot be relied upon by themselves to do more than create a breathing-space during which methods can be devised for eliminating rather than merely continuing to frustrate the potential excesses of demand.

One drastic method for eliminating excess demand, which has been widely used on the Continent, is the compulsory writing-down of the values of money and moneyclaims. This forcible decrease in money and money-claims can be applied, if so desired, in different proportions to different types of holdings or even of holders. If the decreases are sufficiently drastic, and if the Government itself can subsequently so order its finances as to avoid the necessity of re-creating an excessive quantity of money, such a policy can solve at a blow the problem of potential excess demand, though probably only at the cost of much hardship, injustice and perhaps unrest.

If the Government is not prepared to contemplate so drastic a solution of the problem of excessive liquidity in the

private sector, it must rely on other methods. One way in which it could destroy some of the money and money assets created during the war would be to develop a very large budget surplus and use it for paying off debt, including debt to the banks. A similar effect could be achieved by means of a very large special tax on capital. It is true that any very large budget surplus, whether or not achieved by means of a formal capital levy, would in fact be paid largely out of idle balances and the assets circulation. If, however, the greater part of the money withdrawn from the assets circulation were simultaneously returned to it by Government repayments or purchases of Government securities held by the public, and only a small part, together with the payments out of idle balances, were used to destroy money by paying off debt to the banks, the reduction in the public's surplus of money and money assets could be achieved without directly affecting the rate of interest ; for any reduction in the quantity of money in the assets circulation would be offset by the reduction in the quantity of assets to buy with it. Indeed, if the whole of the money withdrawn from the assets circulation were returned to it, rates of interest would tend to fall.

In such a situation, it would probably be better to obtain the surplus with which to pay off debt by a once-for-all capital levy than by very heavy taxes imposed on income, as being less likely to deter effort or divert it to forms less liable to tax. The fact that the payments were made out of idle balances or the assets circulation would not matter, for the purpose of the operation would be to remove the excess of liquidity and so to prevent an unwanted further rise in money incomes rather than to reduce money incomes below the existing level. The position is thus very different from that in which a Government uses taxes paid out of capital for making income-creating payments ; in this case, the effects of the Government's financial operations are on balance inflationary, though less inflationary than if it made income-creating payments out of newly created money borrowed from the banks. It is thus not high taxation as such which tends to be inflationary but the use of such taxation for making income-creating payments ; and it is on Government expenditure rather than on taxation that the emphasis should be laid if it is feared that the payment of taxes out of capital is having inflationary effects.

Even if the Government cannot see its way to an actual reduction in the public's holdings of money and money assets, there are still two ways in which it can limit their inflationary effects while gradually removing direct controls. The first of these is to find alternative ways of limiting income-forming expenditure without raising interest rates. If, for instance, it were possible to persuade persons to spend considerably less than their incomes and so to transfer large amounts of money from the income circulation to the assets circulation or to idle balances, the effect of business transfers of money into the income circulation in the process of replacing physical assets depleted during the war would be counteracted. Unfortunately, the shortage of personal physical assets, the excess of money and money assets already owned by persons, and the redistribution of incomes due to the heavy taxation necessitated by continued heavy Government expenditure combined to reduce personal saving in Britain after the war to a very low level ; and the devoted workers of the National Savings Movement may be congratulated on their achievement in preventing a much larger consumption of wartime personal saving than has in fact occurred.

A more successful method of keeping down personal consumption expenditure has been the restraining of wage increases in the face of rising prices. By this means, wages have been kept generally below the equilibrium level, business profits (before tax) at above the equilibrium level, and incidentally unemployment at a level lower than has probably been achieved ever before over so long a period. A very large part of these business savings has been drained off by high business taxes, and much of the rest retained for the replenishment of business assets, more for the reason of business needs than because of the Government's discouragement of increases in dividends. It would not be far wrong to say that, at least until extremely recently, all Governments in Britain since the war have used businesses

as a kind of involuntary tax-collecting agent, rather as King John is said to have used the money-lenders of his time.

A third possibility is for the Government itself to impose personal taxes of a type likely to be paid out of income, in excess of its own income-creating payments. While in the years from 1948 to 1951 the Government seems to have had total income-destroying receipts (including the proceeds of foreign aid) substantially in excess of its own income-creating payments, a very large part of these receipts were derived from the exceptional profits obtained by businesses. Indeed, the attempt to secure a much larger tax yield at the direct expense of personal incomes would probably have encountered serious economic and political difficulties.

Throughout the period since 1947, the Government has supplemented its efforts to divert money out of the income circulation with efforts to restrict the total quantity of money, or at any rate to keep its rate of increase below that of the real national income. With a constant quantity of money, the amount of money in the income circulation can be increased only by depleting idle balances and the assets circulation. Incomes can therefore continue to rise for a time, but the rise will sooner or later be checked when the rise in the income circulation and the fall in the assets circulation and in idle balances have restored their relationship to equilibrium. The increasing depletion of the assets circulation will bring with it a rise in interest rates, the extent of which will depend on the success of the Government's efforts, referred to above, to return money from the income to the assets circulation. The more successful the Government is in restraining the income-creating expenditure of persons, the greater the proportion of the national resources which can be made available for the increase of physical assets held by businesses or the Government, and the more rapid will probably be the rise in the real national income. But there is no doubt that, if the Government is willing to face the effects on asset accumulation of a sufficiently high rate of interest, an inflation can always be brought to an end simply by action to ensure that the quantity of money does not rise further.

There is one question which still remains to be answered : Do we really want the gradual inflation of recent years to be brought to an end ? Hitherto it has been possible to maintain an abnormally high level of employment by ensuring that the rise in money wages lags sufficiently far behind the rise in selling prices to ensure that the demand for labour, in most occupations and in most places, is in excess of the supply. If the inflation ends and prices cease to rise, wages will tend to catch up, gross profits will return to normal, the excess demand for labour will disappear and unemployment will rise. On the other hand, if even creeping inflation continues here after it has ceased in most of the rest of the world, we may expect more balance of payments crises and probably further depreciations of the currency.

There seem to be only two possible solutions to the problem of reconciling the present level of employment with the absence of inflation. One is the permanent pegging of wage rates at below an equilibrium level, either by self-denying decisions of the trade unions (if they can get their members to follow them) or by Government regulation ; and even if either of these courses should prove possible, they would involve further difficulties in securing and maintaining a desirable distribution of labour between places and occupations. The only other conceivable solution seems to be a continual increase in real output per person employed, so that money wages could continue to rise, without raising costs of output, though always just a little later than when most employers could afford to pay them. Even this, of course, would not prevent a high degree of unemployment in the event of a serious fall in world demand for our exports, or renewed inflation and balance of payments crises if our rising production was offset by a continual worsening of our terms of trade, so that the national real income failed to rise. Nevertheless it seems to offer a chance, perhaps the only real chance, of reconciling full employment with a continued absence of inflation. Let us hope that conditions at home and abroad will enable us to turn this theoretical possibility into an actual achievement.

ENGINEERING IN THE NEXT TWO THOUSAND YEARS

Address by

LORD DUDLEY GORDON

PRESIDENT OF SECTION G

I HOPE it is right to start my Presidential Address by expressing the gratification which I must naturally feel at the honour done me by selecting me as the President of the Engineering Section of the British Association for the current year. I have spent all my working life in the engineering and allied industries and my most youthful recollections are of my desire to be so engaged, but I am certain that I never contemplated that I should find myself in the very honourable office which I am privileged to hold to-day.

I appreciate the compliment paid to me all the more because I have to confess that this is only the second occasion on which I have attended the Annual Meeting of the British Association for the Advancement of Science. The former occasion was as far back as 1897 when the Annual Meeting was held in Toronto. My presence was probably due to the fact that the names of both my parents appear in the list of members for that year.

My most vivid recollection of that meeting was a paper by Professor Roberts-Austen on 'Molecular Movements in Metals.' Certain points were demonstrated by the melting of metals in a small electric arc furnace consisting of a small crucible or porcelain bowl over which an electric arc was formed between two carbon electrodes. Lord Kelvin was present during the reading of this paper and I remember my youthful amazement when at the end of the paper he asked some questions of the operator of the furnace, whose name was Stansfield, as to exactly how it worked. I imagined that the great man possessed such a store of knowledge on all scientific subjects that it would be unnecessary for him to ask questions about anything from anyone.

Looking through the list of eminent engineers who have given presidential addresses to Section G, it is seen that in 1951 Sir Claude Gibb gave his presidential address on the past two thousand years of engineering, and I will make an attempt to look forward instead of backward and to suggest some thoughts of how the science and practice of engineering may develop in future years.

It is perhaps appropriate to do so at the present time because up to quite recent years the energy from which the production of power has been derived has been mainly from heat created by the combustion of some material found in the earth, whether it would be wood or coal or oil. The obvious exceptions are the production of power from water falling from a high level to a lower level, which is in a sense using the force of gravity, or from wind which is the result of atmospheric changes.

The energy made available by combustion of coal has only been turned into useful work as the result of the construction of apparatus which in its turn has depended upon other minerals taken out of the earth. In the early days of the development of the industrial era no particular care was taken to preserve such resources because with the rate of demand which existed at that time, they appeared to be virtually inexhaustible ; but in more recent years it has been realised that at the present rate of progress there is a limit to these supplies. This has resulted in careful investigations being made into the reserves which remain, notably the report by the Materials Policy Commission of

the United States appointed by the President, to consider 'resources for freedom,' known as the Paley Report. This report obviously deals with the matter from the point of view of the resources available in parts of the world not covered by the Communist block, and does not look further forward than the next twenty-five years. Even looking forward over this comparatively short period the report comments that detailed estimates are beyond the capacity of even the most electronic intelligence and we are in fact looking into the fog of the future. This fog of the future is even more mysterious when it is realised that to-day less than 10 per cent. of the population of the world is consuming half the volume of the materials now being produced from the earth's surface. A striking example is the inroads made on the Precambrian Ores in the Lake Superior area where two-thirds of the known supply has already been exhausted. This is coupled with the thought that these deposits are the result of what took place as long ago as a thousand million years and that no replacement is possible. This problem of replacement does not yet apply to timber ; it is suggested, for instance, that the Bowater Pulp Mills in Newfoundland can continue indefinitely at the present rate of output because timber is not being felled at a rate greater than its replacement by new growth.

Minerals which are being mined to-day are from deposits which are reasonably accessible, but the knowledge that they are not inexhaustible has naturally led to prospecting in less accessible parts of the earth's surface. From present knowledge it would appear that these further deposits add greatly to the estimated period during which such supplies will be available for the use of man, but the cost of the necessary development and the transport of minerals to the point where they are made use of will considerably increase.

Estimates as to the length of time that will be taken to exhaust the present resources of coal, which still remains the greatest source for the production of heat, vary as between different parts of the world. The Paley Report, to which I have already referred, estimates, for instance, that at the present rate of consumption, the United States coal resources are good for another 2,500 years, whereas estimates for the coalfields in Great Britain are limited to something like 250 years only. Some estimates are indeed as low as 150 years. This always on the assumption that the increase in the rate of consumption will not ascend even more steeply than it is doing at the present time.

The most important alternative to the use of coal for the production of heat is oil and petroleum products generally. Here the estimates as to reserves available vary more greatly than those for coal, probably due to the fact that new sources of oil are constantly being discovered and developed whereas this does not apply to coal to any great extent. The Paley report suggests that the present known sources in the free world may be exhausted within a generation. This estimate would not be affected by the addition of the resources available in Communist controlled countries, which are unlikely to become exporters because they can do no more than meet their own requirements.

These estimates probably ignore resources which may exist beneath the bed of the oceans. These would have to be developed on the same lines as the deposits already being worked under the sea in the Gulf of Mexico, but even if these deposits do exist, new methods of exploitation would have to be devised at a greatly increased cost. The possibilities of this source of oil are discussed in Miss Rachael Carson's most fascinating book, *The Sea Around Us*.

The other obvious source of fuel is natural gas developed on the lines already in use in Canada and in Italy. It does not appear from information at present available that this source will make any considerable contribution to world sources of heat over the long period under consideration.

Sources such as peat and oil shale over this period are probably not worth considering.

So much thought is given to-day not only by scientists and engineers but also by the general public to the possibility of development of nuclear energy, that less anxiety exists about the question of fuel supplies than would otherwise be the case, but at the present stage of develop-

ment materials such as uranium have to be found and are at present being sought all over the world. This is an interesting example of how material for which there is no important known use in one generation may be in great demand in another. Fifty years ago uranium, by virtue of its close similarity to tungsten, was noted as a possible alloy to be used in tool steel, but not much progress was made. To-day we find the somewhat dramatic development of the creation of Uranium City and Port Radium in Canada, the former I believe being largely constituted from materials moved from the former town of Goldfields.

In South Africa the gold mining industry is paying a great deal of attention to the recovery of uranium which until recent years would have been looked upon as not worth extracting from tailings dumps even if its presence had been realised. The strength of the South African position is that it is worth while working the mines for the gold alone. Gold is in a quite exceptional position compared to any other metal because it is a basic medium in the exchange of currencies, and no completely satisfactory alternative has so far been devised. The mining of gold for its own sake is therefore likely to continue for a very long period, and uranium from this source can be obtained without the heavy overhead expenditure involved when uranium has to be extracted for its own value only.

It would be quite inappropriate in a Presidential Address making this wide survey to attempt to discuss the details of possible developments in nuclear energy. It is perhaps sufficient to say that, so far as we have gone at present, progress has been made in the direction of producing heat which is made use of to create power as an alternative to combustion of fuels and is used in the form of normal heat engines. Maybe there will come some development by which nuclear energy could be used so to speak direct, by the use of apparatus not yet thought of.

At present the development of nuclear energy appears to be based on the availability of uranium and possibly thorium which, like other metals which have to be mined, come from resources which at the present rate of consumption may well be exhausted before the period of two thousand years. There is, however, the thought that by a process of reaction there may be a type of reproduction, the so-called breeder piles,[1] which would greatly reduce the amount of uranium required and so extend the life of the discoverable resources. It may not be outside the bounds of possibility that in the course of development some alternative method of producing nuclear energy might be evolved which would displace the use of uranium.

An example of the expectations which are raised by the present progress towards the development of nuclear energy is well indicated by the suggestion, made some months ago, that the coal industry would be prudent to assume that before long the production of power from nuclear energy will be no more costly than coal and for this reason research should immediately be undertaken with a view to finding an output for surplus coal. On the other hand it has been suggested by Sir Charles Darwin in his book, *The Next Million Years*, that the amount of uranium at present in the earth is such that if it is to remain the only source from which nuclear energy can be produced, it is unlikely to be as much as can and will come from coal. It is unlikely to be ten times as much and certainly will not be one thousand times as much, so that if this assumption is correct, the supply would be exhausted well within the period of 2,000 years which is under discussion.

The use of the so-called hydrogen bomb as a destructive agent has already received some attention, but just as fire which was first used by man for cooking and warming and then as a destructive agent was later harnessed for the production of power, so it is possible that the almost unlimited amount of hydrogen recoverable either from the air or the sea might be used as a source of power. Sir Charles Darwin's book, which I have mentioned, indeed expresses the hope that it will never be possible to release all the ordinary hydrogen in the earth because it might well involve the destruction of our planet. But there may be hopes of successful use of heavy hydrogen as a fuel.

Sources of power which do not depend

[1] Since this paper was written, the existence of a breeder pile in the U.S.A. has been announced.

on the creation of heat by combustion are obviously water power and wind. The attraction of these sources of power is that it is difficult to conceive that they could be exhausted within the next 2,000 years, though there would presumably come a time much sooner than that when all the possible sources of hydraulic energy had been developed. This will surely some day include the use of energy derived from ocean tides. Here is an immense source of power which is inexhaustible. Professor Haldane, however, has sounded a warning that the final result of unlimited development of tidal power would be to increase the braking action of tides very considerably so that the rotation of the earth would slow down and the day would become longer. The moon would again begin to approach the earth and the final result would be the destruction of this planet, but as he calculates that this final judgment is not likely to take place before A.D. 36 million we need not be greatly concerned about it as a deterrent during the next 2,000 years.

Experiments for the production of power from wind are already being undertaken, but in this case it would appear that the constant variations in the amount and velocity of movement of air in any given spot are so great that we cannot yet see how a large and constantly available source of power could be developed.

No attempt has yet been made to use the heat available in the inner part of this planet. There are from time to time spectacular demonstrations in the form of volcanic eruptions which give an indication of the immense resources available. These would have to be tapped by some form of excessively deep bore holes and controlled by some means not yet thought of. The danger to be avoided is obviously the release of forces which would cause some terrible catastrophe similar to the destruction of Krakatoa in 1883.

There remains solar energy as a possible source of power, but again limited as to location and magnitude, a subject on which Dr. Parker of the National Coal Board gave a most interesting paper more than two years ago.

Generation of electricity from water power, wind, tides or solar energy can be undertaken only in those places where nature has made such sources of energy readily and constantly available. They will therefore in the future, if they are to be made of general use, depend on the transmission of power over great distances. Progress has already been made in this direction by the transmission of electricity in the form of direct current at high voltages, but such means of transmission must always be very costly not only from the point of view of the original installation, but also as a result of the high charges for maintenance. This difficulty would be overcome if some form of 'wireless' power transmission could be devised. These intermittent sources of power will become more valuable when some means is found at long last for the large scale storage of electricity.

It seems certain that in future years the progress of engineering science will be more rapid than it was in the nineteenth century for the reason that so much knowledge has been accumulated and that certain principles are so well established that they can no longer be doubted.

I refer for example to the laws of thermodynamics including acceptance of the Carnot principle, as well as to the gas laws of Boyle and Charles. The properties of steam at temperatures and pressures well beyond those in present use are well established, while in another direction we are now at home in the field of sub-zero temperatures. By parallel improvements in instrumentation also, the efficiencies of engines, machines and structures can be more closely studied, with advantage to the economy of constructional materials and of fuel.

There may be other sources of power which are as yet unexplored. I will venture to suggest that the force of gravity may some day be used as a source of power. It is used to-day indirectly in water power generation of energy, but if we can learn how to control the force of gravity itself great developments would follow. If it could be controlled or even generated and its intensity and direction varied at will, it could be used for many purposes, particularly for travel. This suggestion is obviously quite visionary. Knowledge to-day regarding gravitation is slight. It is true that the latest

mathematical efforts have led Einstein and others to believe in a connection rather than a mere similarity between gravitation and magnetic force. No physical meaning to these equations has yet been found and it is possible that as knowledge increases it may be found that there is no hope of the control of this force by the inventions of man.

One of the most important results to mankind in the development of the science of engineering, is the increase in the ease of movement from one part of the world to another. It was not very long after the introduction of the steam engine that it was applied to travel by means of railways and steamships. The same can be said of internal combustion engines and to a lesser degree the steam turbine which has so far been applied for travel purposes almost exclusively to ship propulsion, though there have been examples certainly in the British railway system of steam turbine locomotives.

The increased speed and range of travel which the development of science can provide, has always been a favourite subject of the novelist who makes the future his theme. Jules Verne not only imagined beforehand with considerable accuracy the development of the submarine, but also in his novel *The Clipper of the Clouds* describes something similar to the present-day helicopter. H. G. Wells in his novel *War in the Air* was not quite so accurate in his forecast since he visualised the dirigible airship as possessing greater striking power in war than the aeroplane.

Most of these forecasts assume the use of forces which were well known at the time of writing. Jules Verne for instance in his novel *Voyage to the Moon* imagined a projectile propelled by the explosion of gunpowder in a cannon composed of a vertical bore in a mountain. In the same way to-day proposals for inter-planetary travel assume the use of rocket propulsion on the basis of developments now taking place, but the control of the force of gravity, if it could be achieved, would not only make the return journey more feasible, but would overcome the disadvantages of the varying forces of gravity during the journey and on arrival at planets which, being smaller, have a lower force of gravity than our own.

Another alternative source of power is the use of some chemical action not yet perfectly understood. Working on the principle of thermo-dynamics, engineers attempt to design a prime mover which will use the highest possible temperature of the agent selected and collect the energy which is given off as that agent is reduced to the lowest possible temperature, but in the case of the human or other animal body which exerts power, there is no corresponding absorption of an agent at high temperature rejected at a lower temperature. In the human body for instance energy appears to be accumulated in the form of food, particularly carbohydrates, without any appreciable rise in temperature and when doing work such as turning the handle of a winch or hauling on a rope, no appreciable drop in temperature takes place. When thinking of a period as long as 2,000 years, is it unreasonable to suggest that a source of power based on a more or less isothermal chemical reaction may not be evolved ?

So far as the immediate future is concerned, it appears to be certain that, with regard to mechanical engineering, power units will become almost exclusively those giving power by rotating rather than reciprocating motion. The first known prime mover making use of steam is generally accepted to be the classical reaction turbine of Hero, but when the steam engine began to be developed as a practical working unit in the latter part of the eighteenth century, the reciprocating motion was the one generally employed. Both steam and internal combustion engines were developed on these lines though the greater part of the power they developed was applied through fly-wheels and pulleys to a rotating motion, the obvious exception being the use of reciprocating pumps and compressors driven either by coupling through the crank shaft or directly by tail rod drives.

Steam power was first used on a considerable scale for pumping water which had for a long time been achieved by the plunger type of pump, the power being provided by men or by horses. This may have influenced the lines on which the steam engine was developed, but in any case designers and constructors were limited by the materials then available.

The generation of electricity and the use of electrical power has always been developed through rotating generators and electric motors. It was therefore a natural step forward that the prime mover should also be rotating in the form of either steam or gas turbines. The application of power by rotational movement has long been known in the case of centrifugal pumps and other similar apparatus and in recent years there has been considerable development of turbine compressors for various uses, mainly for the larger sizes. The replacement of the immense blowing engines associated with the early days of blast furnaces, which are to-day superseded by rotary blowers, is an example which immediately comes to mind. There is every indication that before very long this will apply to smaller sizes as well. Gas turbines and jet propulsion for aircraft are already accepted almost as normal and a beginning has been made with similar propulsion for motor cars and ships. It seems therefore quite reasonable to suppose that certainly within the next fifty years reciprocating motion will become the exception rather than the rule.

Turbines and compressors tend to adopt constantly increasing speeds and higher temperatures so that weight and space occupied are constantly being reduced.

Great attention is being paid to-day to increasing the speed and range of air travel and it is confidently expected that long distance travel at supersonic speeds will become common practice within the next twenty years or so. No less an authority than Sir Frederick Handley Page has suggested that the next increase in speed when it does come will be in the form of a considerable jump to something like 900 miles an hour, and even higher speeds are evidently being considered. Certainly there have been references in the Press to the building of so-called flying saucers at these very high speeds. If such advances are to take place, it would seem essential that they should be accompanied by some means of reducing greatly the take off and landing speed of aircraft, not only from the point of view of safety, but also with a view to reducing the size of air-fields and the monstrously long runways which high speeds demand. This would also have the advantage of making it possible to construct airports closer to the great centres of population.

The problem of the reduction in the noise created by aircraft will surely one day be solved, as it has been so successfully in the case of motor cars. Indeed when supersonic speed in level flight becomes the general rule, some means for the elimination of noise will become a vital necessity.

Now all these advances in the science and practice of engineering are the result first of all of the accumulation of knowledge as the outcome of the achievements of the past, but the result of improvements in design and practice can only be made use of as a result of the materials which become available. These new or improved materials again are the result of the accumulation of knowledge being constantly added to by research, and it all goes back to the availability of the mineral resources which came into being millions of years before man was ready to make use of them and are now being so rapidly exhausted—rapidly, that is, in comparison to the age of the world or even the period of the existence of man.

Whatever new sources of power may be developed, they can be made use of only by power producing units which are made of materials which have to be extracted from the earth's surface, or produced in some other way. The power is transmitted to machines in factories which are constructed of much the same materials. The machines in the factories produce goods which again are made of the same materials all taken from the earth's surface. In this connection to-day we think mainly of iron and steel products, though the use of other metals and plastics is becoming more and more familiar.

This leads quite naturally to the consideration of how long existing sources of supply for fuel and minerals are likely to last as demands increase, and what is to take their place when these supplies are eventually exhausted.

The use of iron and steel products, not merely for engineering apparatus, buildings and similar capital goods, but also for consumer goods is so universal that it

seems quite reasonable to measure the standard of living by the consumption of iron and steel and its final products per head of population.

The accompanying table compares estimates of steel consumption per head in various countries (based upon ingot production less direct exports, ignoring 'indirect exports' of steel in metal goods). It illustrates the point made in the report of the Productivity Team representing the Iron and Steel Industry which visited the United States in 1951 :—

> 'It may be said without exaggeration that the American way of life manifests itself in a material sense primarily in the form of goods containing steel.'

There seems no reason to suppose that this tendency to increased consumption of steel will diminish during the next few hundred years. It is reasonable to expect that the standard of living will be gradually raised throughout the world and that even if the present rate of increase in population does not continue as rapidly as it is now doing, nevertheless the total demand will continue to increase.

There are therefore two problems which have to be solved—first the provision of sufficient power to meet world demands, and secondly the supply of materials which will make the application of that power useful to mankind, and it is the science of engineering which will be mainly responsible for solving these problems after the possible solutions have been indicated as the result of research. There is every indication that iron and steel products will continue to be the main source of supply certainly for the next few hundred years, and there is still great scope for development in the production of iron and steel in the direction of improved composition and structure of metals so that the same results can be achieved with a much smaller weight of material. It has been suggested for

Apparent Consumption of Steel per Head
(in ingot lb.)

	Average 1937/38	1948	1950	1951	1952
U.S.A.	641	1,131	1,237	1,343	1,138
Canada	346	623	614	793	737
Sweden	482	625	645	715	
Australia (*a*)	428	415	538	643	
U.K. (*b*)	495	614	630	642	706
Germany (*c*)	596	273	469	497	654
Belgium-Luxembourg	353	757	501	488	527
Netherlands	333	326	370	428	
France (*d*)	289	408	332	407	518
Italy	123	110	141	166	

(*a*) Year ended June 30.
(*b*) Adjusted for stock changes.
(*c*) Includes Saar 1937/38 ; West Germany only 1948–52.
(*d*) Includes Saar 1948–52.
Source : British Iron & Steel Federation.

This table is a clear indication that with improved standards of living for which every country in the world is striving, there must be increased consumption of steel per head for as long as the available sources of raw materials make it possible to meet the demand. The table also indicates that the increase is directly related to improved standards, that is to say, those countries in which productivity is known to be low lag far behind those countries with high productivity.

The generation of electricity and the use of electrical power has always been developed through rotating generators and electric motors. It was therefore a natural step forward that the prime mover should also be rotating in the form of either steam or gas turbines. The application of power by rotational movement has long been known in the case of centrifugal pumps and other similar apparatus and in recent years there has been considerable development of turbine compressors for various uses, mainly for the larger sizes. The replacement of the immense blowing engines associated with the early days of blast furnaces, which are to-day superseded by rotary blowers, is an example which immediately comes to mind. There is every indication that before very long this will apply to smaller sizes as well. Gas turbines and jet propulsion for aircraft are already accepted almost as normal and a beginning has been made with similar propulsion for motor cars and ships. It seems therefore quite reasonable to suppose that certainly within the next fifty years reciprocating motion will become the exception rather than the rule.

Turbines and compressors tend to adopt constantly increasing speeds and higher temperatures so that weight and space occupied are constantly being reduced.

Great attention is being paid to-day to increasing the speed and range of air travel and it is confidently expected that long distance travel at supersonic speeds will become common practice within the next twenty years or so. No less an authority than Sir Frederick Handley Page has suggested that the next increase in speed when it does come will be in the form of a considerable jump to something like 900 miles an hour, and even higher speeds are evidently being considered. Certainly there have been references in the Press to the building of so-called flying saucers at these very high speeds. If such advances are to take place, it would seem essential that they should be accompanied by some means of reducing greatly the take off and landing speed of aircraft, not only from the point of view of safety, but also with a view to reducing the size of air-fields and the monstrously long runways which high speeds demand. This would also have the advantage of making it possible to construct airports closer to the great centres of population.

The problem of the reduction in the noise created by aircraft will surely one day be solved, as it has been so successfully in the case of motor cars. Indeed when supersonic speed in level flight becomes the general rule, some means for the elimination of noise will become a vital necessity.

Now all these advances in the science and practice of engineering are the result first of all of the accumulation of knowledge as the outcome of the achievements of the past, but the result of improvements in design and practice can only be made use of as a result of the materials which become available. These new or improved materials again are the result of the accumulation of knowledge being constantly added to by research, and it all goes back to the availability of the mineral resources which came into being millions of years before man was ready to make use of them and are now being so rapidly exhausted—rapidly, that is, in comparison to the age of the world or even the period of the existence of man.

Whatever new sources of power may be developed, they can be made use of only by power producing units which are made of materials which have to be extracted from the earth's surface, or produced in some other way. The power is transmitted to machines in factories which are constructed of much the same materials. The machines in the factories produce goods which again are made of the same materials all taken from the earth's surface. In this connection to-day we think mainly of iron and steel products, though the use of other metals and plastics is becoming more and more familiar.

This leads quite naturally to the consideration of how long existing sources of supply for fuel and minerals are likely to last as demands increase, and what is to take their place when these supplies are eventually exhausted.

The use of iron and steel products, not merely for engineering apparatus, buildings and similar capital goods, but also for consumer goods is so universal that it

seems quite reasonable to measure the standard of living by the consumption of iron and steel and its final products per head of population.

The accompanying table compares estimates of steel consumption per head in various countries (based upon ingot production less direct exports, ignoring 'indirect exports' of steel in metal goods). It illustrates the point made in the report of the Productivity Team representing the Iron and Steel Industry which visited the United States in 1951 :—

> 'It may be said without exaggeration that the American way of life manifests itself in a material sense primarily in the form of goods containing steel.'

There seems no reason to suppose that this tendency to increased consumption of steel will diminish during the next few hundred years. It is reasonable to expect that the standard of living will be gradually raised throughout the world and that even if the present rate of increase in population does not continue as rapidly as it is now doing, nevertheless the total demand will continue to increase.

There are therefore two problems which have to be solved—first the provision of sufficient power to meet world demands, and secondly the supply of materials which will make the application of that power useful to mankind, and it is the science of engineering which will be mainly responsible for solving these problems after the possible solutions have been indicated as the result of research. There is every indication that iron and steel products will continue to be the main source of supply certainly for the next few hundred years, and there is still great scope for development in the production of iron and steel in the direction of improved composition and structure of metals so that the same results can be achieved with a much smaller weight of material. It has been suggested for

Apparent Consumption of Steel per Head
(in ingot lb.)

	Average 1937/38	1948	1950	1951	1952
U.S.A.	641	1,131	1,237	1,343	1,138
Canada	346	623	614	793	737
Sweden	482	625	645	715	
Australia (*a*)	428	415	538	643	
U.K. (*b*)	495	614	630	642	706
Germany (*c*)	596	273	469	497	654
Belgium-Luxembourg	353	757	501	488	527
Netherlands	333	326	370	428	
France (*d*)	289	408	332	407	518
Italy	123	110	141	166	

(*a*) Year ended June 30.
(*b*) Adjusted for stock changes.
(*c*) Includes Saar 1937/38 ; West Germany only 1948–52.
(*d*) Includes Saar 1948–52.
Source : British Iron & Steel Federation.

This table is a clear indication that with improved standards of living for which every country in the world is striving, there must be increased consumption of steel per head for as long as the available sources of raw materials make it possible to meet the demand. The table also indicates that the increase is directly related to improved standards, that is to say, those countries in which productivity is known to be low lag far behind those countries with high productivity.

instance that structural steel, if more completely homogeneous, could be so greatly increased in strength that the framework of a steel building might present the appearance of a spider-web construction compared to what we are accustomed to see to-day.

Now of even greater importance to the future of mankind than the necessary supply of materials to provide all the equipment which, though starting as a luxury becomes a necessity as the standard of living increases, is the provision of sufficient food which again is in greater demand as the standard of living increases.

Sir Charles Darwin in his book, to which I have already referred, has some interesting thoughts on this subject. Certainly there has been noted the tendency for the rate of increase of population to decrease with an increased standard of living, but even so it seems not unreasonable to assume that the population of the world may double itself in a century. This leads to the interesting conclusion that in 2,000 years the population would have increased a million fold and would require a million times the amount of food at present being produced in the world. It is difficult to imagine that this could be achieved, and long before the period of 2,000 years is exhausted there would be constant struggle to provide by some means or another sufficient food to feed the population.

Now this problem is already beginning to manifest itself and at all events in this country we have become so much accustomed to rationing that it is held as a great achievement when the controls which go with rationing are removed.

Agricultural science is constantly showing how both vegetable and meat supplies of food can be increased, but these methods in almost every case bring engineering into play to provide the solution. One possible means of increasing agricultural resources is the irrigation of deserts which up till now has been achieved by diverting water from river systems, so widely used for many centuries in Egypt and now being developed in India. It is possible that future developments might take the form of converting sea water into fresh water by means of condensation or some other mechanical contrivance on a scale far larger than any method now in use and, by a great development of civil engineering combined with afforestation, irrigate vast areas such as the Sahara and make them available for agriculture.

Broadly speaking it may be said that up till now agriculture has been possible in those regions of the earth where water, evaporated from the ocean and passed in the form of clouds to the land, is precipitated in the form of rain and one of the functions of engineering in the future may be to reproduce that action by mechanical means so that the effect of what would be the equivalent of controlled rainfall could be applied to any section of the earth's surface.

Mention of the sea as a source from which the demands of man can be met makes one turn to some of the suggestions made by Miss Rachael Carson in her book *The Sea Around Us*. Hitherto the sources for the creation of power and its use in the production of materials and finished goods for the use of man, have been sought almost entirely from the dry land, but it is suggested that the ocean is the earth's greatest storehouse of minerals. Apart from the estimated resources of reserves of fuels in the earth, estimates have been made of the reserves of the more important metals, in addition to iron ore. Manganese ore does not appear at the present time to have any great economic reserves, but the Paley Report suggests that improved mining methods can be expected to meet the demand for many years to come. Aluminium as at present produced, depends on supplies of bauxite ore provided there is an ample supply of electricity at a low price, which has led to the development of hydro-electric power for this purpose. No doubt it will be necessary in later centuries to devise some other means of producing aluminium.

The Paley Report attaches the words 'reserves inadequate' for many metals such as chromium, copper, zinc, lead, nickel and tungsten. These are only examples, but it must be remembered as already mentioned, that this report refers mainly to the next twenty-five years, and so it seems certain that more and more attention will have to be paid to the resources available from the sea.

Two important materials are already

obtained in large quantities from the sea. In the case of magnesium it is estimated that four million tons of magnesium are normally present in each cubic mile of sea water. Magnesium is already being extracted from the sea in commercial quantities in Great Britain and may be expected, as development continues, to provide structures of greatly increased strength and reduced weight as compared with what is available to-day.

The other important material already being obtained from the sea is iodine. In *The Sea Around Us* it is referred to as ' the most mysterious of all substances in the sea.'

I have tried to indicate some of the problems which will have to be solved by engineers in the space of 2,000 years. It seems reasonable to assume that we have reached a stage in the development of scientific knowledge which can form a basis for future advance with a diminishing reliance on the principle of trial and error. Thus advances will be made as the result of working out methods of which the final conclusion can be predicted with reasonable accuracy if only materials with the necessary characteristics to make use of the new methods can be produced. It would seem therefore that the present generation can feel that it has not merely a great opportunity but a great responsibility to start now to develop methods which will make it possible for the progress of man to continue when the resources at present available have become exhausted. Failure to commence development on these lines within the next fifty or hundred years would mean that life on this planet within 2,000 years might well deteriorate to a level much lower than that existing to-day. This closing thought is one which above all I wish to impress on the young engineer to-day.

I wish to acknowledge the great help I have received from Dr. S. Main in collecting data and preparing notes for me. I also thank Mr. E. W. Colbeck for suggestions he has made to me and similar thanks are due to Mr. H. S. Peiser.

ANALYSIS AND DESCRIPTION IN SOCIAL ANTHROPOLOGY

Address by

PROF. MEYER FORTES

PRESIDENT OF SECTION H

PRESIDENTIAL addresses to this section of the British Association have been some of the most notable landmarks in the development of the anthropological sciences. This is not surprising when we recollect the names of those who gave them. Haddon (1905) and Rivers (1911), Seligman (1915) and Radcliffe-Brown (1931) could not fail to make important additions to knowledge and advances in theory on the occasion of a presidential address to Section H of the British Association. But in science and scholarship, unlike letters and the fine arts, advances made by leading personalities are much more the decisive steps in a collective effort than the fruits of pure inspiration. Did not Isaac Newton say that he stood on the shoulders of giants? When we look back to two such seminal events in the history of social anthropology as Rivers's presidential address of 1911 and Radcliffe-Brown's of 1931, we see how true this is. Entangled in the controversies of the day, Rivers thought he was defending the so-called historical point of view which was so soon to fizzle out in the diffusionist extravagances of Elliott-Smith and Perry. Yet he was already being swept into the current of ideas which brought about the revolutionary transformation of social anthropology summed up in Radcliffe-Brown's address of 1931. He was struck by what seemed to him to be the remarkable observation that the indigenous social structure of a people often maintains itself in the face of greatly changed political conditions, and in spite of the disappearance of their material culture and their religious customs and beliefs. Ten years later the current of thought he was indirectly responding to took massive shape in the first monographs of Malinowski and Radcliffe-Brown (1922); and if one wanted to sum up briefly one of the chief results of the field research inspired by Malinowski and Radcliffe-Brown, one could say that it lies in the understanding we have now reached of the full implications of Rivers's remark. In undertaking the office you have honoured me with I put my trust in the precedent I have just illustrated. Never before has social anthropology in this country been so vigorous and productive as in the post-war decade just drawing to a close. My task will be well fulfilled if I can present some aspects of this collective enterprise.

Radcliffe-Brown's presidential address of 1931 was memorable for the formulation he gave in it of the principles of theory and method which have inspired the best field work in social anthropology during the past thirty years. In his own words, this method and theory 'looks at any culture as an integrated system and studies the functions of social institutions, customs and beliefs of all kinds as parts of such a system.' The triumphs of ethnographic field work during the past thirty years have been the result of approaching the tasks of description and analysis of field data in terms of this principle.

We can see what a difference this approach has made to social anthropology since Rivers's day if we compare his justly famous study of the Todas (Rivers, 1906) with the first, and still unrivalled masterpiece of functionalist ethnography, Malinowski's *Argonauts of the Western Pacific*. The comparison is instructive because if any ethnographic study can be regarded as a precursor of Malinowski's it is surely that of Rivers. Did he not state, at the

very beginning of his book : ' The whole of Toda ceremonial and social life forms such an intricate web of closely related practices that I rarely set out to investigate some one aspect of the life of the people without obtaining information bearing on many other wholly different aspects ' (p. 10) ? But instead of seeing this as a discovery calling for a new way of regarding custom and social organisation, Rivers thought of it as an impediment in his task of separating out the different aspects of social life. He thought that scientific method required him to keep apart the description of the facts of observation and the theories by which he explained the facts. He dealt with the ' facts ' by enumeration and description ; his explanations or theories were tacked on to them and were couched in the idiom of the time. So, to take an example at random, he interprets the fact that a woman after child-birth undergoes ritual associated with the dairy as possibly relics of a time when women had more to do with the dairy than at present (p. 330). For Malinowski the inter-connectedness of the customs and institutions of the Trobrianders was the discovery which determined his handling of the observed facts ; but in addition, following a more sophisticated view of scientific method, he refused to separate fact from theory. Every way in which facts are grouped in description involves theories, implicit or explicit, about the connections between them that are significant ; and significance is a function of the kind of questions to which the observer seeks an answer. The reality of social life in any society is a welter of human activities, material objects and natural events. Science, which Norman Campbell, I believe, somewhere calls the greatest of the arts, works by selecting from the welter and seeking to establish relationships between the data selected. Criteria of relevance, rules of procedure and forms of communication are presupposed in this. And behind the selection, arrangement and presentation lie the kind of questions asked by the investigator (cf. Kaufman, Ch. V ; and Nadel, *passim*).

The new ethnography introduced by Malinowski and Radcliffe-Brown is quite explicit on this point. Ethnographic facts are meaningless, except in the colloquial sense of compatibility or incompatibility with the observer's commonsense categories, unless they are examined in the light of theory. The ethnographic monograph of to-day, as Prof. Evans-Pritchard points out (1951, Ch. V), is generally a study of a problem—that is, an investigation of an hypothesis—as well as a record of field data. Of course the ethnographers of the past, for all their pretended neutrality in matters of theory, in fact held quite definite theories as I have already suggested. Every general term they used, even such innocent looking ones as ' family ' and ' clan,' was loaded with theoretical assumptions. But the deliberate effort to make a field study both an investigation of hypotheses and a means of arriving at new hypotheses that is our practice to-day, is a recent development.

This is one of the chief features that distinguishes functionalist ethnography from its precursors. It is epitomised in the reproach often cast at Malinowski, that he generalised about humanity at large from his experiences among the Trobrianders ; but criticism has not stopped his successors from continuing in this habit. And one reason for this is that the results have handsomely vindicated the procedure of testing, amending and adding to the generalisations which make up the body of social anthropological theory by intensive study of one society at a time. For it is a method more in line with some experimental procedures in the natural sciences than any of the versions of the comparative method, which is commonly supposed to be the best way of reaching valid generalisations in social anthropology. It resembles the kind of experiments made with a single species of plants or animals classically typified by Mendel's work on sweet peas.

Functionalist ethnography has been so fruitful in matters of theory because the principles which guide it in looking for significance in ethnographic observations require an analytical, as opposed to a descriptive mode of investigation. It is worth adding that comparative studies which have been of theoretical value, not just accumulations of parallels and illustrations, likewise follow analytical procedures. This was brought out in Prof.

Forde's presidential address to this section (1947), and the example he used, ' the character and role of kin groups in social organisation,' is particularly apt. It is obvious in Prof. Radcliffe-Brown's recent defence of the comparative method (1952). Comparison is a secondary step in establishing a generalisation. Seeing that the number of human societies, past and present, is finite and small, we can, by comparison, establish the range within which a proposition concerning a general tendency is valid ; the hypothesis itself is commonly derived by the study, in the first instance, of customs, institutions, or social relations in a single social system.

To explain what I mean by analytical methods let me first note that, in order to distinguish units of custom or of social organisation, the ethnographer has to isolate standard and recurrent features of social situations (cf. Nadel, Ch. V). When I say that among the Tallensi a first-born is prohibited from eating fowl I am making a statement based on the analysis of a representative sample of information about first-borns and of observations of their behaviour in appropriate situations. But this level of analysis is fairly elementary, though too often taken for granted without more ado, as I have explained elsewhere (1949). For practical purposes it can be regarded as description ; and though every analytical step in ethnography opens up questions of theoretical significance, the higher the level of analysis the more important is the theoretical problem thrown up, since the level of analysis is related to the diversity of the descriptive data referred to.

The essence of description is that observations are grouped together in accordance with their actual relationships and contexts of time and place. Ethnographic literature is so largely descriptive that it might seem superfluous to give examples, but as I am going to refer to marriage customs and institutions later on, it may be useful to remind ourselves how these are dealt with by the method of description. Marriage is described as a sequence of customary activities following one another in a set pattern. The description generally begins with some account of heterosexual interests in children before they reach marriageable age. There probably follows a brief reference to initiation or puberty ceremonies where these occur ; then a description of the normal customs of courtship and betrothal, with an aside about the proprieties regulating social relationships between the different parties during the betrothal period. There follows an account of the wedding rites and celebrations, and finally a discussion of the legal formalities such as the payment of bride price. To round off the account there may be some consideration of divorce and other ways of terminating marriage.

This formula is filled out in varied ways by the addition of graphic detail and commentary but it has remained essentially unchanged for fifty years. It is sometimes given an air of theoretical respectability by calling marriage a *rite de passage* or by lumping it in with kinship institutions. While this brings marriage into relation with the wider field of social organisation of which it is a component institution, it still follows the sequence and concomitance of things as they actually happen. Marriage is a stage in the journey through life ; or it is the prelude to the setting up of the family. The approach is that of the natural historian.

Analysis takes a different course, and its nature is most easily seen when it occurs, as commonly happens in ethnography, in a descriptive context. For we can see then that its aim is to find answers to questions of a different order from those that receive descriptive answers.

Analytical questions refer to what Whitehead called ' ideal isolates ' by contrast with the heterogeneous welter of empirical data ; and ideal isolates are theoretical constructions. The analytical method is to break up the empirical sequence and concomitance of custom and social relations and group the isolates so obtained in categories of general import. As it is an indispensable step in the movement from field observation to theory and back to the test of further field observation, it has often been implicitly followed by anthropologists ever since field research began.

Description cannot yield generalisations ; we can arrive at generalisations only by way of analysis. But for this, the ideal isolates we use must have meaning

in terms of the descriptive reality of social life, and the isolates we use in the analysis of one class of anthropological facts must be consistent with those we use in the analysis of any other class. In other words, our isolates must form a theoretical system. It is through positing isolates that have no counterpart in social reality, or are inconsistent with the general body of valid theory, that many plausible anthropological hypotheses have come to grief. Advances in social anthropology depend on devising the right kind of isolates. Rivers was an acute observer. But he confounded the isolates he worked with in his most ambitious theoretical study, *The History of Melanesian Society*. He correctly perceived that ' distinctions in (kinship) nomenclature are . . . associated with distinctions in conduct ' (vol. 1, p. 45). But instead of following up rigorously this synchronic correlation, as Radcliffe-Brown so profitably did, he introduced a contradictory and empirically useless isolate in the form of the hypothesis that kinship nomenclatures contain ' anomalies ' which are relics of extinct marriage and sex customs.

The most promising tendency in social anthropology to-day lies in the development of analytical methods and isolates within the framework of the functionalist hypothesis that the customs and institutions of any people make up a system of interdependent parts and elements, which work together to maintain the system in a steady state and have value for the realisation of legitimate social and personal goals.

I want to emphasise that customs and institutions are not isolates but descriptive units. When we speak of ' the family ' or of ' sacrifice ' we are referring to descriptive not analytical units. But when, to quote two rather different classical examples, Malinowski wrote of ' reciprocity as the basis of social structure ' (1932, Ch. IX) and Radcliffe-Brown discussed the functions of kinship institutions in distinguishing, ensuring and transmitting different kinds of rights over things and in persons (1935) they were developing isolate concepts. This is clear from the fact that Malinowski, for example, tried to establish the validity of his generalisation by examining a range of descriptively diverse units of custom, and claimed that it held over ' the whole culture and the entire tribal constitution ' (1932, p. 49). Isolates are abstractions (cf. Evans-Pritchard, 1951) and it is characteristic of abstractions that they may vary greatly in generality. The isolate ' lineage ' discussed in Prof. Forde's presidential address is of less generality than the isolate conceptualised in the phrase ' rights *in personam* ' as used by Radcliffe-Brown, which may include rights of a chief over his subjects or of a priest over adherents to a cult.

The argument can be put simply in this way. In analytical terms we think of a custom, an institution, a belief, a social relationship, any unit of actual social life, as if it were made up of a number of isolates, and we think of an isolate as if it were an element that enters into the formation of different kinds of customs, institutions and social relations within one society. And our isolates must form a theoretical system because we have to define them in relation to one another. Lineage descent is defined by distinguishing it from such other isolates in the same system as kinship filiation; rights *in personam* are defined by contrasting them with other kinds of rights and with their negative, the absence of rights. Furthermore, our isolates must pass the fundamental test of a good theory, which is that it makes possible generalisations that bring together and explain empirical observations not previously seen to be related to one another. Such very general concepts as that of the integration of a social system or that of the consistency of a people's culture are meaningful as isolates at a high level of abstraction.

However, there is no argument so strong as a good example, and this is what I want to turn to. Recent studies of marriage in African societies are particularly illuminating. Since a survey of the data has just appeared (Philips, *et al.*, 1953) I will not attempt a general review but use one or two specific examples to illustrate my argument. Theoretical discussion of the institution of marriage in Africa has centred mainly on the widespread custom by which a payment is made by or on behalf of the husband to the wife's kin. Thirty years ago Junod in his well-known ethnography of the Thonga (p. 121, vol. 1,

1927 edition) explained this custom as follows :

' To understand them, we must observe that marriage in primitive or semi-civilised tribes is not an individual affair as it has become with us. It is an affair of the community. It is a kind of contract between two groups, the husband's family and the wife's family. What is the respective position of the two groups or families ? One of the groups loses a member, the other gains one. To save itself from undue diminution, the first group claims compensation, and the second grants it under the form of the lobolo. This remittance of money, oxen or hoes will allow the first group to acquire a new member in place of the one lost, and so the balance will be kept. This conception of the lobolo as a compensation, a means of restoring the equilibrium between the two groups is certainly the right one.'

This explanation, with modifications, reappears in many subsequent analyses of marriage, especially in African societies which have corporate patrilineal lineages. Professor Radcliffe-Brown, for instance, in his recent exposition of his theory of kinship and marriage (*Introduction* to Radcliffe-Brown and Forde, 1950) agrees that from one aspect ' the marriage payment can be regarded as an indemnity or compensation given by the bridegroom to the bride's kin for the loss of their daughter ' (p. 50), and he then refers to those societies—of which there are many in Africa—where the daughter's bride price is used to marry a wife for her brother or some other male of her natal family. But, as he emphasises, this accounts for only one aspect of African marriage payments. Another and often more important aspect is that marriage signifies a transfer to the husband and his kin of certain rights in relation to the wife and the children she bears. Thus the full meaning of formalities carried out at the time a couple begin to cohabit and set up house is apparent only in terms of the structure of the family that results some years later.

The idea that marriage in primitive society is basically a form of exchange derived in the last resort from the general principle of reciprocity, is also developed with great learning and brilliance by Professor Levi-Strauss (1949) ; and though his primary interest is in cross cousin marriage he extends his hypothesis to include marriage of the type common in Africa. It is not possible to do justice to the subtlety and many-sidedness of Professor Levi-Strauss's analysis in a few sentences, but for purposes of my present argument it is enough to pick out his contention (p. 582) that bride price payment is a means of bringing about deferred and roundabout exchanges of women for women in a society made up of many exogamous groups.

It is unnecessary to list all the writers who have based ethnographical or comparative study of African marriage essentially on the idemnity (or exchange) hypothesis for the methodological point to be obvious. Descriptively, it is undoubtedly the case, in patrilineal societies at any rate, that one ' group of kin ' (not further specified for the moment) ' loses a woman ' and through the medium of the bride price ' recovers a woman.' Analytically considered it is not so straightforward, as Radcliffe-Brown shows by the qualifications he adds to the exchange hypothesis. The transfer of rights in a woman, let alone in her as yet unborn offspring, is a different thing from an exchange of woman for woman. The concept of rights refers to an isolate, to a special kind of social relation between two parties which is enforceable by sanctions. As soon as we put it this way we see how different is the level of analysis from that involved in reducing marriage to a special form of exchange. The patient observer can almost literally see bride price being exchanged for a woman. To arrive at ' rights ' he has to examine the whole range of social relations in which the married couple have joint and separate roles and also consider marriage as a developing process through a stretch of time.

This is well brought out in a paper by Dr. Laura Bohannan (1949) which sums up the analytical method of handling this problem. In this paper Dr. Bohannan examines Dahomean marriage with a view to finding out if there are any common principles behind the thirteen different ' types ' described by the ethnographic authorities. She concludes that they are all different ways of arranging the distribution of rights in a woman, the two kinds of rights at issue being those held in a woman as wife (*in uxorem*) and those held in her as to the children she may bear (*in genetricem*).

These rights can be conjoined, or can be vested separately. In one type of marriage, for instance, which would descriptively be classified as a 'matrilocal' variant of the more normal 'patrilocal' arrangement, what analysis shows is that only rights *in uxorem* are transferred to the husband in return for the bride price, the wife's lineage retaining rights *in genetricem* —that is, jural control of the offspring of the woman thus married. In the case of the royal lineage rights over the children of daughters are never transferred but the women are given in marriage, with transferred rights *in uxorem*, to the King's political favourites. This explains how in Dahomey, as in many other parts of Africa, a woman of wealth can 'marry' a woman. What happens is that the wealthy woman acquires rights in the reproductive powers of a young woman and hands over rights in her as wife to the man who is brought in to father the children. What is specially interesting is that the bride price gives a man rights only over the marital and domestic services of his wife; a ritual act in which the lineage ancestors are informed of the marriage is necessary to confer rights over the offspring. It is significant also that the distribution of these rights *in personam* is closely related to the various ways in which rights of inheritance and succession are distributed and vested.

When we look again at the descriptive data in the light of these analytical concepts many things that otherwise seem not to be of great moment take on significance. Thus even in patrilineal societies in Africa a woman is never completely severed from her natal family and descent group by marriage. As I have described in detail for the Tallensi (1949) and others have shown for other African peoples, a woman never forfeits her status as daughter in her patrilineal lineage and family; but rules of residence, reinforced by very strong sanctions of the ancestor cult ensure that this status is never confounded with that of wife. To put it in terms of the reciprocals of Dr. Bohannan's isolates, a woman's rights as daughter (*qua filia*) are not extinguished by the rights she acquires as wife and mother (*qua uxor et mater*). This is one of the reasons why the mother's brother plays so important a part in the kinship systems of peoples with patrilineal lineages. In short marriage is not an arrangement for exchanging women but a process of transferring—or perhaps exchanging—rights, the parties and commodities involved being each a focus for a cluster of rights which do not necessarily remain combined.

This initial step in analysis opens up a number of interesting possibilities. What we isolate as a right in jural terms we see as vested in persons, as embodied in structural relations, and as symbolically expressed in ritual beliefs. Or we can link this chain of isolates in the opposite way and end by asking how rights are sanctioned. We are then led to the discovery, excellently illustrated in Prof. Mayer's analysis of Gusii bride price (1950), that the bride price has an element of a liability to those who receive it, since it is at their service only in proportion to the degree to which the woman for whom it is given adequately fulfils the obligations of wife and child-bearer.

But we must turn to societies in which rights in property, persons, political office and cult are vested in matrilineal descent groups to see how fruitful analytical methods can be by comparison with a purely descriptive approach. A number of studies of marriage in African societies in which matrilineal descent is the critical factor in jural and ritual relations have been published. I would draw attention, in particular, to Prof. Forde's monograph on Yakö marriage (1941) and to Dr. Richards's important account of Bemba marriage (1940) as well as to her more recent analysis of what she has aptly called 'the matrilineal puzzle' in Central Bantu societies (1950). In the latter paper Dr. Richards discusses some of the researches of Belgian scholars, who have devoted much effort to the study of marriage among the matrilineal peoples of the Congo. For my present argument, however, it will be more satisfactory to use my own field data from Ashanti, rather than to rely on these or other published studies. I have referred to Prof. Forde's and Dr. Richards's papers because my analysis has much in common with theirs.

The late Dr. R. S. Rattray compiled the most important descriptive facts about the customs and institutions of Ashanti

marriage in his great series of monographs more than twenty years ago (1923–29). In the rural areas of Ashanti these customs and institutions are unchanged in essentials. But if we want to understand Ashanti marriage analytically it is not enough to consider only the sequence of customary acts that normally lead up to a marriage. The relevant isolates cannot be properly evaluated without taking into account the whole range of institutions involved in lineage structure, domestic organisation and kinship. I can only give some indications to-day.

Ashanti marriage is singularly lacking in the vivid and symbolic rites and ceremonies so common among Bantu peoples. It is a purely jural transaction. But certain features of this transaction are the result of the Ashanti concept of the personality as expressed in ritual notions. Every Ashanti belongs by right of birth to his mother's matrilineal lineage. This automatically confers rights of citizenship and domicile in a chiefdom, as well as rights of inheritance and succession in matters of property and office vested in the lineage. The Ashanti lineage is a localised group of great solidarity, the men and the women ranking equally in economic and domestic affairs but the women being excluded from direct participation in the religious and political activities that are tied to the lineage. This solidarity is symbolised in the very stringent prohibition of sex relations between members of a lineage, breach of which is regarded as a sin and a crime and was formerly punished by death. The corporate unity of the lineage is the fundamental principle of Ashanti law, morals and religion (Fortes, 1950).

Lineage membership gives the individual unconditional rights to use productive resources held by the lineage, to seek any office vested in it, to claim legal guardianship from it, and to have the ritual protection of the ancestors and gods of the lineage. These imply the right to assistance from the lineage in distress, to share in its prosperity, and most important of all, to be given a proper funeral by it. Correspondingly, the lineage as a corporate body has the right to the loyalty of its members in political and legal matters, to economic contributions from them in a common cause such as the funeral of a member, and to unconditional support in maintaining the resources, rank and reputation of the lineage. The individual cannot abjure membership of the lineage for without it he is jurally a nonentity. But the lineage can, with the unanimous consent of all members, expel a member who has consistently flouted lineage obligations, in particular a member who has infringed its moral solidarity by committing incest or seducing the wife of another lineage member.

The strength of a lineage lies in the number of its members. Clearly, therefore, its most vital asset is the reproductive powers of its female members. Control over these cannot be alienated, and this is a decisive factor in marriage. But there is another side to this matter. Paternity carries a very high value. A free person is born legitimate, that is, with the rights arising from lineage membership, no matter how or by whom he was begotten. He derives no legal rights from his father. And yet, it is both a disgrace and a disability not to have a father who has properly acknowledged paternity, even if he has not brought one up, as a father should. In descriptive terms this goes back to the Ashanti conception of the personality, to which I will return in a moment. Analytically it is connected with the great dichotomy in Ashanti life between all the social ties that spring from matrilineal kinship and all those that spring from the association of a man and a woman in marriage. The former only become fully effective in adulthood, of which the most important sign is socially recognised fitness to marry ; the latter are of great weight for childhood, when education and character formation are taking place. The former are mediated by the sibling bond between mother and mother's brother and are focussed in the jural authority of the mother's brother ; the latter are created through the conjugal bond of mother and father.

The Ashanti conception of the personality is in part a reflection of this dichotomy. Very summarily, every person has in his make-up a spiritual or temperamental element which comes to him from his father at conception and is symbolised

in an hereditary ritual bond shared by close patrilineal kin. He also has an individual character that is partly a matter of fate and partly of upbringing; and finally he has the jural personality derived from his lineage status. A child does not thrive if his father's soul does not watch over him with care and affection nor can he become a person of worth and virtue if he is not taught the proper skills, manners, morals and beliefs by his parents during childhood. Thus a child cannot grow up in the right way if he has no father to care for him (see references in Fortes, 1950).

Conversely, a man's life is unfulfilled if he has no children. As a member of his lineage his concern is to have sisters' sons; as a man his chief aim in life is to have children. That is why impotence or sterility is as great a humiliation for a man as barrenness for a woman. The jural expression of this attitude is the father's right to give his child the name by which it will be distinguished from other members of its lineage. The moral expression of this is the child's duty to contribute to the support of the father in his old age, and, especially, to share with his other children the expense of providing his coffin when he dies. It is as disgraceful for an Ashanti to die without children to buy his coffin as it was for a Victorian Englishman to have a pauper's funeral. Last, but not least, there is the strength of paternal sentiment which is so often shown in the gifts *inter vivos* made by a father to his children (see Rattray, 1929). One further item of descriptive information is pertinent. Ashanti do not like marrying outside their native town or village. They give many reasons for this but its most important effect is that husband and wife can, if they live in the same village, continue to reside with their respective maternal kin without detriment to their marital relationship. It is quite common for them not to have a household of their own (see Fortes, 1949). This graphically documents the hold of matrilineal kinship on the individual and the fact that the conjugal bond straddles two lineages.

We can now consider the formalities necessary for a proper marriage in Ashanti and see what they mean (Fortes and Kyei). They consist of gifts, formerly often in kind—palm wine, antelope meat, cloths, etc.—now generally in money. The gifts pass, in the usual African way, from or on behalf of the husband to the bride's kin and the bride herself. There are customary standards of appropriateness for these gifts but the amounts vary according to the circumstances of each case. The tendency is for the amounts to be larger the higher the rank or social standing of the bride's maternal kin or father, but they are never very large. The kinsfolk involved are the father and mother, the lineage head and lineage elders, sometimes the brothers, of the bride and bridegroom. Friends of each family, who may or may not be kin, also play a significant part.

Two conditions are essential before any of the gifts that are jurally critical can be offered. Firstly, the bride must have undergone the public ceremony of declaring her nubility which all girls formerly went through on reaching the menarche and most girls who have not been to school still go through. This marks her attainment of adulthood. It is a sin and a crime, as heinous as incest, for a girl to conceive before this ceremony. Both she and her lover are driven out of the village. The parallel with incest is instructive. The point is that a woman's reproductive powers which come with sexual maturity, are not her own to dispose of at will. The community recognises the existence of these powers and her maternal kin proclaim their control of them.

The second condition is that the girl's parents and lineage elders, especially the latter, must consent to the marriage, and, in the ideal case, the boy's too. On the jural side this is necessary for the gifts to be given and accepted by those who have the rights *in personam* which empower them to do so in binding form. On the personal side this enables the girl's maternal kin to assure themselves that her offspring, their future heirs and successors, will be worthily fathered, and the man's paternal kin to assure themselves that their names and spiritual affiliation will be passed on to children of a virtuous and reputable lineage. It is worth noting that, though he has no jural powers in the matter, the consent of the father is particularly desirable for both spouses, and

records of several hundred extant marriages show that it counts almost as much as the consent of the mother's brother, who represents lineage authority in this context. This is a recognition of the spiritual affiliation a person has with his father and of the moral debt due to him for educating and bringing up his child.

The formality which makes a marriage legal is the passage of what is usually translated as the ' head rum (*tiri nsa*).' This is a gift of drink, either palm wine or imported liquor, or cash in lieu. It is often described as a thanking gift (*aseda*). Drink is given in exactly the same way by the recipient whenever rights in property or office or persons are ceded or conferred. It marks the transfer, before witnesses, as final.

In marriage, the drink is taken by representatives of the bridegroom's lineage head and elders to the head and elders of the bride's lineage. The latter sends half to the bride's father. The remaining half is ceremonially shared by the representatives of the two lineages and the bridegroom. The bridegroom declares that he accepts responsibility for any debts his wife may incur and agrees that treasure trove she may find belongs to her lineage. The marriage is now jurally ratified. Judging by a sample of nearly 300 extant marriages contracted over a period of thirty years (Fortes and Kyei) this formality is carried out in 90 per cent. of marriages. In the remaining 10 per cent. of cases no formal gifts whatever have passed ; but they include a number of couples who have, with the consent of their respective kin, lived as husband and wife for many years and have several children.

The difference between a proper marriage and cohabitation is that in the former case the husband is entitled to damages from any man who commits adultery with his wife, and will also receive an apology from her and her lineage ; in the latter the husband has no redress if his wife is unfaithful. Again, in marriage with ' tiri nsa ' the husband can insist on his wife's carrying out such domestic services as cooking and helping him on his farm ; in the latter he cannot. So cohabitation can only be a stable arrangement if the spouses and their kin have great confidence in one another.

Where ' tiri nsa ' has been paid the husband's paternity of the children of the union is taken for granted, since he is their presumed physical father, but he must all the same give the customary gifts and perform the naming and other ceremonies by means of which he accepts and claims paternity. A man establishes paternity in exactly the same way in a cohabitation union or, if he wishes, in the case of a child born of a casual liaison.

To complete the picture I must add that divorce is easy and common at all stages of marriage and parenthood, and case histories show that it is more often the wife than the husband who initiates the action. The main grievances are incompatibility, neglect of domestic duties by the wife, insufficient support by the husband, and gravest of all, sterility or impotence. A divorce must be formally ratified in the same way as a marriage, only in reverse. The wife's lineage head returns the ' head rum ' to the husband's lineage head and elders with the same ceremony as took place when he accepted it. Whether or not there are children of the marriage is irrelevant.

Two other gifts from the bridegroom are of interest. One is the gift of a sheep or its money equivalent to the bride's father. This is for him to sacrifice to his soul, which has kept spiritual guard over the girl from childhood and is now asked to bless the marriage. The other is a small gift to the girl's mother to console her for the loss of her daughter's companionship. These are non-returnable gifts.

Finally, there may at any stage of the couple's life together be a transaction of a different kind. The husband may be asked to make a loan of any amount called ' head money ' (*tiri sika*) to the wife's lineage. It is found in about one-third of extant marriages. It is actually a kind of mortgage, the security being the wife's fidelity. The wife has a stake in the welfare of her lineage. By consenting to the loan she pledges herself, both to her husband and her lineage, to perform faithfully her duties as a wife. Repayment can only be demanded if failure on the wife's part leads to divorce or when she dies ; and if the wife has been dutiful and faithful for many years, or till death,

the husband may make a free gift to her or her lineage of the loan. Whereas the 'head rum' provides no sanctions on a wife's conduct, 'head money' exploits the wife's lineage bonds to bolster up her marital loyalty. This is neat evidence of the normal condition of mutual exclusiveness, if not opposition, of the two loyalties.

So much for the husband's gifts. It is worth noting that no equivalent returns are made from the wife's side. The reason, which will be clearer from the analysis I shall presently offer, is that the husband's lineage do not have to divide their rights in him with his wife. But there is one ceremonial act by the wife which is of significance. Soon after the marriage is consummated she comes with her mother and sisters to the husband's home, and with their help cooks a rich and elaborate meal known as 'the great dish' (*aduan kese*). This is presented to the husband and his kinfolk and friends in sign of the wife's future role as cook and housekeeper.

It is easy to see that Ashanti marriage customs and institutions form a coherent pattern which closely resembles those reported from other African peoples who reckon descent in the matrilineal line. But in order to explain the pattern we must examine the data analytically. In terms of jural isolates, the individual in Ashanti can be regarded as a cluster of rights *in personam* distributed among corporate groups and persons which are not only structurally distinct but may even come into conflict. These rights correspond to distinct interests and fields of social relations symbolised in different ritual concepts and moral rules. They create divided loyalties and represent different kinds of claims which the individual may make on others. Marriage, parenthood, residence rules, domestic organisation, and the concept of the personality all fit together in a consistent pattern in the light of this formula.

Marriage, as Prof. Radcliffe-Brown has often pointed out, is everywhere a potentially hostile act by each spouse against the kin of the other. In Ashanti this is particularly marked with respect to the wife. For it is in her children that the cleavages between the rights, interests, and loyalties tied to matrilineal kinship and those that arise from paternal filiation, are focussed. There are psychological reasons why an Ashanti woman, like African women in general, never thinks of refusing the responsibilities of adult sexuality, marriage and motherhood. But in accepting them she puts herself and her lineage under an obligation to the father of her children. There are the germs here of an attachment between husband and wife which might endanger lineage solidarity and the rights and claims established by birth and upbringing.

Thus an important feature of Ashanti marriage customs is the affirmation of all existing rights in, and claims on a woman before she enters upon her marital role. This is why the consent of both parents and the lineage elders is required. Again, these rights and claims must be formally admitted by the husband and this is one reason for the gifts. It explains why the gifts are all consumed. The father's sheep, in particular, is not returnable since his rights and claims do not change with changes in his daughter's marital status. By sacrificing the sheep to his soul he accepts the new interests and bonds of marriage and motherhood which will soon come to rival his daughter's attachment to him. This is final whether or not she remains with her husband. The mother's *personal* claim on her daughter's companionship is treated in the same way. Her *jural* rights in her daughter are merged in those of the lineage.

But the crucial issue is the division of rights and claims between the wife's lineage and her husband. The 'tiri nsa' settles that. The fact that it is consumed on the spot and that it resembles the thanking gift which seals a transfer of rights in property, shows, as clearly as the words spoken on the occasion, that it is not a compensation for the loss of a woman of the lineage or a means of bringing about an exchange of a daughter for a wife. It is an agreement about the division of rights in the woman *qua uxor* and *qua filia*. The question of rights in *genetricem* does not arise since these are inalienably vested in the lineage and are subsumed in the woman's obligations as a daughter of the lineage. In a sense it can be said that the 'head rum' creates rather than transfers the husband's rights in his wife

but the procedure on divorce shows that there is an element of lineage control over them. This is connected with the right of the lineage to pawn a member—in the case of a woman, virtually to mortgage her to her husband for a loan—for the benefit of the group, and with the corresponding obligations of supporting needy members, materially and ritually, in life and of performing the funerals of all members.

What then are the rights conferred on a husband by the ' head rum ' gift ? First and foremost, the sole right to the sexual services of his wife, then the right to her domestic services as cook and housekeeper, (symbolised in her cooking of the ' great dish ') then to her economic services in his work of earning a living. He does not acquire any right to take his wife away from her maternal kin or to decide where she shall live. He gains no jural rights over her children begotten by him. But the fact that he is their physical father gives him a moral claim, backed only by ritual sanctions, to establish his paternity. Furthermore, these rights entail the obligation of identical return services to his wife, epitomised in his responsibility for any debts she incurs. Debts result from economic activities and after marriage a woman's working power is, in theory, devoted entirely to helping her husband and so contributing to the support of their young children. Debts stand for the antithesis to lineage ties for they cannot arise between lineage kin, who, by definition hold all possessions jointly. The manner and incidence of divorce gives empirical confirmation of the equality of spouses in the marriage relationship.

The rights retained by the lineage are symbolised in the declaration about treasure trove. The maxim applies to men as well as women. Such a find is a stroke of luck. It might happen to anybody, child or adult. It therefore accrues to the benefit of those who have final jural authority over and responsibility for the finder throughout his life. It signifies simply that matrilineal kinship remains absolutely binding on a woman no matter how she disposes of her physiological capacities. The concept of the person as the focus of a cluster of rights and interests which may be distributed among different fields of social relations is clearly brought out in this rule.

One last point. As is well known from Rattray's works, Ashanti have a preference for cross cousin marriage. Our analysis indicates why. For as I have shown elsewhere (Fortes, 1950) cross cousin marriage is the simplest way of reconciling the divergent and potentially conflicting rights and interests set in action by marriage. Only by stretching the term almost to a point of meaninglessness could it be regarded as a form of exchange. As the Ashanti put it, if a man's sister's son, who is also his heir, marries his daughter, then his property is eventually used to help support her and her children. His obligations to his sister's son are thus reconciled with his affection for his daughter. If a man's son marries his sister's daughter, then his property eventually passes through his sister's son to his son's son, who will very likely be named after him and has the same soul deity as he has, and this is an even better reconciliation of his conflicting obligations and interests. These statements have a metaphorical as well as literal meaning and confirm what I have said about cross cousin marriage.

The analytical concepts and methods I have illustrated by this brief account of Ashanti marriage customs and institutions are common currency in social anthropology to-day. They have been most successfully used in the study of social and political organisation, economic institutions, and law. The next step must be to find out how we can use them in the study of those more complex and baffling aspects of culture, religion and mythology, art and music, technical knowledge, medicine and rudimentary science.

References

Bohannan, L. (1949). ' Dahomean Marriage : A Revaluation.' *Africa*, vol. xix, 4.

Evans-Pritchard, E. E. (1951). *Social Anthropology.*

Forde, D. (1941). *Marriage and the Family among the Yakö in South-Eastern Nigeria.* L.S.E. Monographs on Social Anthropology, No. 5.

Forde, D. (1947). ' The Anthropological Approach in the Social Sciences.' Presid. Add. Sec. H, Brit. Assoc., *Adv. Sci.*, No. 15, p. 213.

Fortes, M. (1949a). *The Web of Kinship among the Tallensi.*

Fortes, M. (1949b). ' Time and Social Structure : An Ashanti Case Study.' In *Social Structure. Studies Presented to A. R. Radcliffe-Brown*, edited by M. Fortes.

FORTES, M. (1950). 'Kinship and Marriage Among the Ashanti,' in Radcliffe-Brown and Forde, *cit. inf.*

FORTES, M. and KYEI, T. E. (1945). Unpublished Field Data of the Ashanti Social Survey, referred to in Phillips *et al.*, *cit. inf.*

HADDON, A. C. (1905). Presidential Address, Sec. H, Brit. Assoc. Adv. Sci., *Ann. Rept.*, p. 511.

JUNOD, H. (1927). *The Life of a South African Tribe.*

KAUFMAN, F. (1944). *Methodology of the Social Sciences.*

LEVI-STRAUSS, C. (1949). *Les Structures Élémentaires de la Parenté.*

MALINOWSKI, B. (1922). *Argonauts of the Western Pacific.*

MALINOWSKI, B. (1932). *Crime and Custom in Savage Society.*

MAYER, P. (1950). *Gusii Bridewealth Law and Custom.* Rhodes-Livingstone Papers, No. 18.

NADEL, S. F. (1951). *Foundations of Social Anthropology.*

PHILLIPS, A., MAIR, L., and HARRIES, L. (1952). *Survey of African Marriage and Family Life.*

RADCLIFFE-BROWN, A. R. (1922). *The Andaman Islanders* (by A. R. Brown).

RADCLIFFE-BROWN, A. R. (1931). 'The Present Position of Anthropological Studies,' Presid. Add. Sec. H., Brit. Assoc. Adv. Sci., *Ann. Rept.*, p. 141.

RADCLIFFE-BROWN, A. R. (1935). 'Patrilineal and Matrilineal Succession,' reprinted in Radcliffe-Brown, *Structure and Function in Primitive Society*, 1952.

RADCLIFFE-BROWN, A. R. (1952). 'The Comparative Method in Social Anthropology,' *J. Roy. Anth. Inst.*, vol. LXXXI.

RADCLIFFE-BROWN, A. R., and FORDE, D. (1950). Editors, *African Systems of Kinship and Marriage.*

RATTRAY, R. S. (1923). *Ashanti.*

RATTRAY, R. S. (1927). *Religion and Art in Ashanti.*

RATTRAY, R. S. (1929). *Ashanti Law and Constitution.*

RICHARDS, A. I. (1940). *Bemba Marriage and Present Economic Conditions.* Rhodes-Livingstone Papers, No. 4.

RICHARDS, A. I. (1950). 'Some Types of Family Structure Amongst the Central Bantu,' in Radcliffe-Brown and Forde, *cit. sup.*

RIVERS, W. H. R. (1906). *The Todas.*

RIVERS, W. H. R. (1911). 'The Ethnological Analysis of Culture,' Presid. Add. Sec. H., Brit. Assoc. Adv. Sci., *Ann. Rept.*, p. 490.

RIVERS, W. H. R. (1914). *The History of Melanesian Society.*

SELIGMAN, C. G. (1915). Presidential Address, Sec. H., Brit. Assoc. Adv. Sci., *Ann. Rept.*, p. 651.

QUALITY AND QUANTITY OF PROTEIN FOR MAN AND LIVESTOCK

Address by

DR. D. P. CUTHBERTSON

PRESIDENT OF SECTION I

THE problem of quality and quantity of protein is ever present in our daily speculations about what we shall eat and wherewithal we shall be clothed. The scale of agricultural production to meet our needs has always been a matter of concern.

HISTORICAL INTRODUCTION

The great French physiologist Magendie was probably the first to draw attention to the need for nitrogenous substances in the food in organic form (1816). Boussingault (1836)—chemist and farmer—while subscribing to the essential nature of nitrogenous nourishment for animals pointed out that every nitrogenous vegetable nutritive substance is generally accompanied by other organic and inorganic substances many of which concur in nutrition. It was in 1839 that the Dutch chemist Mulder suggested the name ' protein ' for what he termed ' unquestionably the most important of all known substances in the organic kingdom.' He also considered that plant proteins must be as effective as meat in replenishing the blood and flesh of the body. Through the influence of Liebig it was thought at that time that protein was the source of muscle energy and it was not until the famous climb of the Faulhorn—an Alpine peak 8,803 ft. high—by Fick and Wislicenus that the death blow to the theory was administered. These men showed that the protein used up in the ascent yielded far less energy than was expended. Later Hirschfeld (1890) showed that heavy exercise will not produce a much greater nitrogen excretion than occurs during resting if the increased energy needs are covered by non-nitrogenous nutrients. Voit (1867) criticised the over-emphasis on protein current at the time and presented his view that protein, carbohydrate and fat form the dietary trinity.

The nutritional imperfections of the protein gelatin and some other proteins came to be appreciated and the association of the imperfection with the lack in these proteins of certain constituent units —amino-acids. Later there grew up the conception of differing biological values amongst proteins (Thomas, 1909), and as a result of the analytical methods of Emil Fischer and others it was coming to be known that though the number of proteins is very large, yet they are constructed out of some twenty different units—the amino-acids—whose position and preponderance in the molecule varies with each kind of protein and gives to it its peculiar properties. The proteins of the food are thus the exogenous source of the amino-acids or specific groups required by the body. As a group the proteins are relatively stable and it is their physico-chemical characteristics which are of great assistance in cooking and baking and make for variety in attractive texture.

The outstanding investigations of Hopkins and his co-workers (Hopkins and Cole, 1901 ; Willcock and Hopkins, 1906), in this country and Osborne and Mendel (1912) in the United States first clearly demonstrated the existence of the indispensable amino-acids. The British workers were able to state ' Protein products (of digestion) may function in other ways than in the repair of tissues or in supplying energy. It is highly probable that the organism uses them, in part, for more specific and more immediate needs.' They had in mind hormones and enzymes. Thus there developed the idea of dietary

dispensable and dietary indispensable amino-acids arising from the digestion of protein in the alimentary tract.

The isotope experiments of Schoenheimer (1946) and subsequent investigators have pointed to the metabolism of protein as consisting of at least two main phases. In the first the dietary protein digestion products (or amino-acids synthesised in endogenous reactions) are incorporated into tissue protein and a portion of the dietary derivatives are catabolised, the intensity differing considerably in the various organ proteins. Then in the second phase, the amino-acids ' displaced ' during the first phase undergo a multitude of metabolic changes. Their degradation may lead to the resynthesis of the same or a different type of protein constituent, to the formation of carbohydrate, fat, or other nitrogenous substances or to the conversion of the ultimate products for excretion.

Nutritionally Essential and Non-Essential Amino-Acids

From this historical introduction we can now regard the contemporary position. We note that the process of digestion of proteins ultimately provides the organism with amino-acids and possibly also simpler peptides, the ' Bausteine ' or building-stones from which it can create or renew the fabric of its tissues, its enzymes, hormones and other protein and non-protein nitrogenous constituents. These products of digestion are absorbed and there seems to be fairly general agreement that free amino-acids, present in rather large amounts in most tissues, are at least in part the physiological precursors of protein, and the incorporation of free amino-acids into proteins has been amply demonstrated by isotopic techniques. If we may generalise from the work of Hokin (1951) and others the evidence appears fairly conclusive that all the amino-acids that are components of a particular protein must be present simultaneously in a tissue before net synthesis will proceed smoothly.

The rapidity of growth of a species is dependent on the percentage of protein in the milk of that species. The total amino-acid requirements of an animal of any age or species are determined by the proportions of the essential amino-acids contained in the tissue being currently formed, or being currently catabolised. Thus the relative prominence of keratin synthesis for the growth of the integument (hair, wool, feathers, etc.) in the young animal and the need for replacement in the adult animal, must be set against the development of muscle and the viscera during growth and the replacement needs for these tissues and for reproductive needs in the adult.

I shall not recapitulate the history of the discovery of the significance of the constituent amino-acids of the proteins but you will remember that the products of hydrolysis of the mixed food proteins in the alimentary tract are usually some eighteen to twenty different amino-acids of which Rose (1938) found that ten, namely *iso*leucine, leucine, lysine, methionine, phenylalanine, threonine, tryptophan, valine, histidine and arginine, are nutritionally necessary for growth of the rat, but that for nitrogen equilibrium in the adult animal arginine does not appear to be essential in the diet as it can be synthesised within the organism at a rate sufficient to meet the needs of the adult organism. There is still some controversy about histidine also being in this same category. Methionine, one of the sulphur-containing amino-acids, if suboptimal in amount, can be partially replaced by cystine but cystine cannot perform all the functions of methionine. Similarly tyrosine though not essential can in part replace the essential phenylalanine. No other interrelationship has been established in which the presence of a non-essential amino-acid decreases the requirement of an essential one. It seems fairly definite that these ten essential amino-acids even in optimal proportions cannot support normal growth of immature rats. Although improved growth has been obtained by the addition of unessential amino-acids, it is generally assumed that the remaining non-essential amino-acids can be synthesised as required by the body, provided that there is a sufficiency of utilisable nitrogen and not too clamant a need. The nutritionally non-essential amino-acids resulting from digestion are presumably available for synthesis into new

protein if they are not used primarily for energy or other purposes.

While there are considerable concentrations of free amino-acids in tissue juices, it is notable both in animals and plants that these are not present in the same proportions as would be required for protein synthesis ; the large concentrations are of amino-acids that are mostly ' non-essential ' for animals and known, both in animals and man, to be closely associated with the metabolism of carbohydrates. The ' essential ' amino-acids are normally present in low concentrations, which suggests that efficient mechanisms exist for making use of them.

Studies on the amino-acid requirements of pigs were delayed until purified rations could be developed that would support normal growth. Now there is evidence of a requirement of tryptophan, arginine, lysine, leucine, *iso*leucine, methionine, phenylalanine and valine in growing pigs and it seems safe to assume that the requirements for pigs will be comparable to those for rats.

Albanese (1950) has stated that human infants probably do not require arginine and histidine from amongst the ten amino-acids essential to the young rat, and Rose has indicated that this also holds for the young male adult. Progress has been such that Rose (1949) has been able to put forward tentative allowances of the essential amino-acids for man and has pointed out that invariably the exclusion of an essential amino-acid from the food of the human subject leads to a profound failure of appetite, a sensation of extreme fatigue, and a marked increase in nervous irritability (Rose, Johnson and Haines, 1950).

Grouping of the proteins into ' complete,' ' partially complete,' and ' incomplete,' as has been done by some observers on the basis of ability of a protein to support growth or maintenance in the rat, requires care in interpretation, for ' complete ' proteins may behave as ' partially incomplete ' when fed in seriously reduced proportion. Quality and quantity cannot properly be separated.

On the basis of their varying ability to permit growth under defined conditions in terms of body weight or nitrogen retention in the rat, bird, or man himself, proteins have been given biological values. These have been of some comparative value in compounding rations.

Supplementation

No protein is normally eaten by itself and the individual proteins in foodstuffs tend to supplement each other in terms of the proper balance of nutritionally essential amino-acids. While it is plain that natural articles of food may contain proteins whose amino-acid contents differ in such manner that these proteins supplement each other, it does not follow that total protein mixture of one natural food is always as efficient as that of another. It is believed that these differing efficiencies ascribed, for example, to milk and grain proteins are mainly due to differences of chemical constitution in terms of essential amino-acid make-up. For a properly proportioned ration of grain and milk may show high nutritive efficiency, doubtless because milk proteins are rich in lysine and tryptophan which are the amino-acids most effective in supplementing the proteins of the cereal grains. But it is also possible that these efficiencies may be influenced by differences in certain known nutrients, e.g. riboflavin, vitamin B_{12}, or as yet unidentified essential nutrients. The so-called animal protein accessory food factor is probably essentially vitamin B_{12}, but recent observations suggest that tissues like liver may supply as yet unknown accessory food factors and that the term ' Animal Protein Factor ' is still not fully defined. And so it is that Sherman (1952) advises that in order to free ourselves from the bias of a demand for high animal protein we should bear constantly in mind : (1) dietaries whose protein is predominantly from cereals and other foods of vegetable origin need only about 10 per cent. of their protein as milk to make the protein and amino-acid intake excellent for maintenance and good for growth ; (2) while a higher percentage of well-selected animal protein induces more rapid growth in experimental animals, it is an open question whether the highest attainable growth is the most desirable ; (3) advantages believed to have been gained from enrichments of diets in their animal protein content may actually owe their improvement to increased intake of

riboflavin, or possibly in part, of vitamin B_{12}, or of folic acid, or both.

It has been found that methionine is the limiting amino-acid in the protein mixture of peas but that with 0·3 per cent. of added methionine (and correspondingly with any food that supplies the needed amount of methionine) the protein of peas becomes highly efficient in nutrition (Woods, Beeson and Bolin, 1943).

The habit of reserving the term ' first class ' for animal protein and ' second class ' for vegetable protein is thus seen to be an obvious oversimplification, for we now know that several plant proteins, and particularly mixtures of them, are as efficient as some animal proteins. It is necessary to note, however, that the observations of Whipple and his school (Whipple, 1942) indicate that for the rapid reformation of blood plasma proteins following their loss from the body animal proteins are in general more efficient than vegetable proteins and thus support the view that animal protein seems more readily adapted to the needs of the animal body in convalescence.

Much has even been said of leaf proteins and more recently still interest has been aroused by the adaptation of the alga Chlorella to culture for the production of protein and oil (Milner, 1951). The proportions of the amino-acids in true protein of the leaf are certainly favourable to the requirements of omnivores, vary very little with season and species, and from amino-acid analysis would appear to put the leaf proteins into the category ' first class ' (although there is a tendency for methionine to be low, as in so many other proteins). Many difficulties have still to be overcome before the extraction of leaf protein for the use of non-ruminants, including man, can become a practical and economic source of dietary protein.

The amino-acid analyses of Chlorella protein showed that it contained all the amino-acids essential for man and totalled about 42 per cent. of the protein. Chlorella fat is similar in general to other highly unsaturated plant fats and should be suitable for food. High capital costs may preclude the development of this and similar algae.

Success in compounding rations for non-ruminant livestock is based on the optimal use of vegetable and, where necessary, animal products so as to provide the essential amino-acids in the right proportions and amounts as well as vitamins of the B-group. Evans (1952) has shown that the proteins in extracted soya-bean meal are inferior to the proteins in white-fish meal for supplementing the proteins in barley meal and fine bran for the pig. Reviewed against the earlier work of Woodman and Evans (1951) decorticated groundnut meal is inferior to extracted soya-bean meal. But it is interesting to note that as good results, from the standpoint of live-weight gain, economy of food conversion and nitrogen retention in young pigs, were obtained by Evans with the vegetable protein concentrates as with the animal protein concentrate although the level of vegetable protein needed to be considerably higher than the minimum level of fish meal protein needed for optimal performance. This seems to cast doubt on the need for ' animal protein factors ' in the diet of bacon pigs, at any rate under the conditions of their experiments, and unpublished work at the Rowett Institute has confirmed their findings.

The work of Woodman and Evans with practical rations is in complete conformity with the idea that the pig's requirements for amino-acids are similar to other species. Their ranking of supplementary proteins to those in cereals is in striking agreement with the findings of my colleagues Drs. Duckworth and Carpenter on the chick.

Special Requirements of the Bird

The requirements of the bird, particularly in respect to its feathering (feathers contain 10 per cent. cystine), growth of flesh and egg production, are considerable and vary according to size, age and breed. Because of its relatively small size we now know almost as much about the bird as we do about the rat in terms of specific amino-acid requirements. For rapid feathering the growing bird needs glycine in addition to the ten essential amino-acids necessary for the rat and even in adult life it still requires arginine, glycine and probably also proline as it cannot synthesise these sufficiently rapidly to meet its needs.

In addition to their content of known vitamins and possibly of vitamins yet to be discovered, animal by-products are valued for poultry as sources of protein rich in those essential amino-acids which tend to be lacking in quantity generally in vegetable diets. Although there is an increasing amount known about the quantitative needs of the young bird for growth, information about the hen's need for amino-acids to meet the demands of its productive potential is still incomplete, though from the qualitative point it does seem that most of the amino-acids essential for the growth of the chick are also essential for egg production in the adult bird.

The evaluation of proteins of diverse origin as sources of amino-acids for egg production is not simple. Results at the Rowett Institute and elsewhere show that rations capable of supporting full egg production can be constructed from feeding stuffs exclusively of plant origin. But as my colleagues Carpenter, Duckworth and Ellinger (1953) have pointed out, it cannot be concluded from this information that plant and animal proteins are of equal value to laying birds. These workers have also stressed that in many practical trials designed to test the interchangeability of protein supplements in the rations of birds (and other classes of farm stock) the level of protein in the ration as a whole has been above requirement, and the presence of this excess of protein above bare requirement invalidates any comparison. Rations devoid of an excess of protein must be used if valid comparisons of feeding stuffs as sources of amino-acids for laying are to be made. Since the pullet at lay still has to gain about one pound in live weight the ration has to cover this need for continued growth as well as for egg production.

As already mentioned, among fowls the very young are required to divert a particularly large proportion of their sulphur amino-acid intake to the rapid growth of feathers which contain as much as 10 per cent. of cystine. The rate of feather protein synthesis as compared to body protein synthesis may determine the sulphur amino-acid requirement for maximal growth. The smaller the bird the relatively greater the need for sulphur amino-acids (Almquist, 1952).

It has been found by Hegsted *et al.* (1941) that a rapidly feathering breed (Leghorns) showed a marked growth and feathering response to arginine and glycine supplements while a slowly feathering breed (Plymouth Rocks) showed little response. They concluded that the rate of feather formation is a primary factor in the arginine and glycine requirements of young chicks.

Since lysine is present in considerable concentration in muscle and organ tissues the faster the growth the greater the requirement for this essential amino-acid. The requirement of the chick is 4·5 per cent. of the protein at normal protein levels and 5·5 in the case of the turkey.

The Requirements of the Ruminant

In the calf, while the stomach system is still essentially simple, the process of protein digestion and the requirements for essential amino-acids in the food are the same as for other simple stomached mammals. But with the development of the rumen and its microbial population the fate of food protein is vastly more complicated than in the monogastric animal.

It is now widely recognised that rumen micro-organisms contribute abundantly to the supply of protein digested in the abomasum (that part of its tract which corresponds closely to the stomach of man and the pig) and small intestine, and that ruminant animals are in consequence less susceptible to critical deficiencies of individual amino-acids in their food proteins than are the rat, the bird, the pig or man. The known synthetic powers of micro-organisms can be shown by the synthesis *in vitro* of high-quality microbial protein from non-protein nitrogenous compounds in the rumen (McNaught *et al.* 1950). The experiments of Agrawala, Duncan and Huffmann (1953) on calves indicate that an appreciable synthesis of protein may occur in the bovine rumen after feeding urea. Many experiments on older animals have been undertaken in this direction, mainly using urea as nitrogen source and the limits within which this approach yields results of practical value are now fairly well understood.

The converse aspect, that micro-organisms can degrade food protein to non-nitrogenous compounds of little value to

the animal, has received much less attention. Pearson and Smith (1943) showed that casein and gelatin were degraded when incubated with rumen liquor *in vitro* and suggested that the value of a protein to ruminants may depend on the extent to which it is degraded in the rumen. McDonald, working with sheep, showed *in vivo* that ammonia is an important product of such degradation (1948a, 1952), and that it may be absorbed directly into the portal blood stream (1948b) and that different proteins may be converted to ammonia in the rumen at very different rates (1948a, 1952). He was able to show that the lysine-deficient protein zein, which is converted very slowly, if at all, to ammonia, nevertheless is partially replaced by lysine-containing protein, presumably of microbial origin (1948c).

Nitrogen-balance experiments at the Rowett Institute have shown that casein supplements fed to hill ewes on a low plane of nutrition in the later stages of pregnancy were used very inefficiently despite their need for protein (Cuthbertson and Chalmers, 1950). In confirmation of McDonald (1948a, b, 1952) we have found extensive conversion of casein to ammonia to occur in the rumen, with absorption of ammonia into the blood stream (Chalmers, Cuthbertson and Synge, 1953). We also found that casein administered by duodenal fistula was better utilised than when administered by ruminal fistula. The course of dissolution of casein in the rumen was observed to be delayed when the casein was in the form of tough lumps. It was found possible to process casein in a way that led to better utilisation as shown by nitrogen-balance experiments. This processed casein gave less ammonia in the rumen.

This formation of ammonia in the rumen from proteins may be an important factor in determining their usefulness to the animal. Processing may have effects on the value of a protein for ruminants quite different from those on the value for non-ruminants. The tendency to value proteins for ruminants solely in terms of their digestibility is criticised and it is suggested that casein has disadvantages as a 'standard protein' in nutrition experiments with ruminants.

We thus see that in the ruminant a much more complex system of protein digestion and metabolism exists.

Further discussion on quality of protein will now refer essentially to the needs of non-ruminants, man in particular.

Alterations in Protein Quality during Processing

The experimental work done in this field is vast and refers chiefly to man. In general it can be stated that in the processing of foodstuffs the sensitivity of amino-acids to destruction by heat must be kept continually to the forefront and means sought to avoid their destruction. But the protein of certain legumes, in particular the soy bean, is improved by heating in the presence of water and it is now known that about half of the growth-inhibiting effect is due to a 'toxic' protein soyin and the remainder to an effect which can be counteracted by trypsin (Liener, 1953). In every case where the depressing effect of the trypsin inhibitor has been most marked there has been a deficiency of total sulphur-containing amino-acids inherent in the protein (Almquist, 1951).

As to the effects of heat on the nutritive value of the proteins of grain products it may be stated that in general the nutritive value of cereal protein is impaired by heat treatment, damage becoming increasingly more marked as higher temperatures are used, the effect being more in diminishing the functional value of the amino-acids within the animal's tissues than a reduction in digestibility of the protein. Outstanding in its liability to heat damage is lysine, particularly in the presence of reducing sugars. It has been suggested that lysine complexes are found which are not readily hydrolysed by enzymes and that such, though absorbed, may not be available for physiological use. If a lysine supplement is baked in bread to enhance the biological value of this foodstuff, it is found that there is inactivation of the lysine in the baking (Bloch *et al.*, 1946; Cremer *et al.*, 1951). Pressure cooking the dough does not inactivate lysine (Peters *et al.*, 1950).

Methods of processing more drastic than baking—such as are necessary to make 'puffed' and 'exploded' breakfast cereals, 'toasted' and 'rusk' products—seriously impair the nutritive value of

cereal in protein for man, in a manner that is largely corrected by adding lysine. Assays with rats have confirmed this (literature reviewed by Duckworth, 1952).

Isolation of the Toxic Factor in Agenised Flour

Unless flour is stored for some months it must be given an improver treatment—to simulate the effects of long storage—so that it will yield good bread and be easy to manage in the dough stage by the baker. For the past twenty-five years the improver most widely used both in this country and North America has been a gas nitrogen trichloride (commercial name 'agene') used at the rate of about 50 p.p.m. When Sir Edward Mellanby in December 1946 published his observation that agenised flour fed in large quantities to dogs and certain other animals gave them a form of hysteria there was naturally much concern amongst millers as to the nature of the factor responsible. Accordingly the problem was given first priority in the research programme of the Cereal Research Laboratory under Dr. Moran. With a strong team he attacked the problem from various angles and isolated the factor ahead of his American colleagues. The progress of the isolation in which Campbell, Work and Mellanby also played a part (1951), took three years, was published at intervals and ultimately it was shown that the factor was a derivative of methionine—methionine sulphoximine—incidentally confirming the suggestion put forward some two years previously by this team. The sulphoximine, a compound hitherto unknown, was soon synthesised (Moran, 1952).

Fortunately, despite extensive tests in this country and the United States, there is no evidence that flour treated with nitrogen trichloride is harmful to man. It is, however, the policy of the Government to discontinue its use as soon as a suitable alternative has been found. One such is another gaseous chemical—chlorine dioxide—which has replaced nitrogen trichloride in the United States and Canada. Nevertheless, despite its adoption after extensive tests in these two countries it has been considered by Moran and his group desirable to extend the American work in certain directions and particularly to acquire first-hand knowledge of the chemical interactions of chlorine dioxide with flour. The results so far published indicate the production of modified amino-acids as the result of treatment but quantitatively so small that the reduction in the content of the parent amino-acids is negligible. The degree of oxidation of the unsaturated fatty acids by chlorine dioxide falls within that observed in flours which have been lying untreated for three months. Whether or not the oxidation products may have an anti-vitamin E activity (Moran, Pace and McDermott, 1953) remains to be determined. I trust that we shall have an even better improver.

Regenerative Processes Following Illness

The disturbance of protein metabolism following accidental injury, surgical procedure, and acute illness may sometimes be profound, leading to considerable losses of nitrogen, sulphur, phosphorus and potassium. I will not digress further into this interesting field for I have reviewed the effects of trauma elsewhere (Cuthbertson, 1950, 1953), and in any case Mr. Wilkinson, who follows, will discuss this in more detail. I would add, however, that such disturbances must cause a partial shift in amino-acid needs of the adult to something akin to a growth phase and under circumstances where the natural response is geared to be independent of food supply, for the severely injured animal cannot go in search of food.

The Newer Methods of Estimating Amino-Acids in Foodstuffs

Protein chemists have been only too conscious of the analytical errors in the old classical methods of amino-acid analysis due to losses in the course of fractionation and purification, or to incomplete extraction. My colleague Synge (1953) has pointed out that peptides may be carried down with the proteins. Further, as the relative amounts of the separated proteins may not be accurately known, the amino-acid content of a given food protein is estimated rather than the entire food it comes from. As already pointed out, the various protein fractions of a food product usually have, individually,

less biological value than the entire product because of mutual compensation. In order to determine all the amino-acids which foodstuffs contain, new methods of determining amino-acids were required. For this purpose it became necessary to hydrolyse the entire product as it presented itself in its original form, without previous extraction and separation of the various nitrogenous components.

In the more recently developed microbiological procedures attempts have in fact been made, but it was always felt desirable to develop a chemical method which would be equally adaptable to small quantities. In collaboration with Dr. S. Moore, Professor Bigwood and his co-workers in Brussels have elaborated the method of Moore and Stein (1951) whereby substances are greatly diluted with concentrated HCl before hydrolysis and this reduces considerably the reactions that may take place between amino-acids and interfering substances such as carbohydrates. The method is based on column chromatography of the amino-acids on ion exchange synthetic sulphonated resins, and shows great promise, forming one of the most useful advances in quantitative chemistry in our time.

The investigations by this group of Belgian workers on cassava are particularly important and I shall digress to discuss them at greater length.

Cassava meal (manioc) is the staple food of many native populations in Africa and elsewhere, and covering practically all their caloric requirements, but it is particularly low in protein (1·5 to 2 per cent.) and high in carbohydrate (more than 90 per cent.). The methionine content of cassava is very low and that of its cystine conspicuously low (traces which barely allow reliable quantitative determination). When the natives for whom cassava is the staple foodstuff, in particular young children, are examined, the most striking disorder is kwashiorkor, a name first given by Williams (1933), which has as its chief characteristics : (*a*) retarded growth in late breast feeding, weaning and post-weaning periods ; (*b*) alteration in skin and hair pigmentation ; (*c*) oedema ; (*d*) fatty infiltration ; (*e*) heavy mortality if better and more protein is not supplied (Brock and Autret, 1952). Although the aetiology has not yet been fully established, many regard it as due to protein deficiency. Dean (1952) has found that concentrated milk protein gave best results, but mixtures of plant proteins were also effective. Brock and Autret bring evidence to show that freedom from kwashiorkor which is enjoyed by many largely vegetarian countries, may depend on nothing more profound than the suitable mixing of plant materials in the diet. If this is correct it indicates a relatively easy method of prophylaxis and adds strength to the hope that the cure of this disorder in the tropics will not depend on a supply of animal protein, for that would be wellnigh impossible meantime.

Cereals as Sources of Protein

The dominance of our staple cereals as the chief energy sources of our diet means that they are also important sources of protein and they are therefore worthy of particular study. As my colleague Duckworth (1952) has pointed out, it is fair to say that one of the main—though often unrecognised—tasks of nutritional research has been to discover the extent to which cereals as sources of proteins and vitamins need supplementing with other foods to give a balanced diet for man and for his non-ruminant stock—pigs and poultry.

Variations in the protein content of cereals through climate, fertiliser or genetic effects are probably of importance where the level of protein intake is critical, but when the intake of cereal protein is some 30 g. out of 78 g. total protein per head per day even in households of the lowest social class recorded in the Ministry of Food's Domestic Surveys, such differences are generally of little or no importance because of the high safety margin. Moreover such differences are largely reduced during the blending of wheats from diverse regions as they are drawn together into the hands of the miller, finally the slight increase in protein content of flour that results from raising the extraction rate from 70 per cent. up to 100 per cent. is in the light of the German studies of McCance and Widdowson, also most probably without practical importance even to growing children eating very large amounts of the corresponding breads

and liberal supplies of vegetables. This generalisation is probably true even after the processing reductions in lysine availability, drastic though these are in some manufacturing processes and in such household practices as making toast, for only a small fraction of the day's total is so treated. The average domestic consumption of liquid milk in the lowest social class described in the Ministry of Food's annual report for 1950 by itself provides a little more lysine than is the minimum daily requirement of normal man as assessed by Rose (1949). But as doctors we must always be aware and take care of those who are poor or who eat poorly.

Coeliac Disease

When on the subject of cereals, it is of interest to note that our knowledge of coeliac disease has recently been clarified considerably by the finding that the removal of wheat and rye flour from the diet of children with coeliac disease has resulted in rapid improvement both clinically and biochemically (Dicke, 1950), and that deterioration follows the reintroduction into the diet of wheat flour or wheat gluten, but that wheat starch has no effect (Anderson *et al.*, 1952). As reported by Weyers, van de Kamer and Dicke at the First International Congress on Dietetics at Amsterdam, the decisive factor is the protein gliadin.

Total Protein Allowances for Man

A survey of the diets of well-developed countries throughout the world (Cuthbertson, 1940–1941) showed that in most diets the percentage of the total energy represented by the protein component of the diet is about the same, generally between 10 and 14 per cent. though tending to rise higher in more northerly latitudes.

An inspection of consumption levels in the U.K. permits the following generalisations to be made. The percentage of the total calories in the diet represented by its protein moiety remains a very constant fraction irrespective of social class and level of energy expenditure, the range for the means of the different groups not falling outside the values of 11 to 13 per cent. The constancy of this proportion of energy represented by dietary protein has existed in this country for most of the century, and while the total energy and protein intakes were naturally affected by unemployment during the depression of the mid-thirties this proportion was unaffected by unemployment. If anything, the general level has risen very slightly within the period of food rationing.

The evidence produced by Johnson and Kark (1947) that U.S. soldiers in various theatres of war ranging from the tropics to the arctic, consume food in which the proportion of calories represented by protein also occupies a constant proportion of the total calories, indicates that this constancy of the proportion of protein is not altered by considerable climatic variation. Since the natural diet of the peoples of the arctic and sub-arctic regions contains a higher proportion of protein it would seem that habit played a considerable part in the dietary pattern of the men studied by Johnson and Kark.

Pre-war dietary studies of children whose intake has *not* been restricted by financial stringency also showed at all ages this same remarkable constancy in the proportion of protein, the values ranging from 13·2 at one year to 12 at eighteen years of age (Widdowson, 1947).

During pregnancy the proportion of calories derived from protein is also found to remain constant and at about the same level as the general population (Thomson, 1953).

When asked by the Committee on Nutrition of the B.M.A. for his views on protein in the diet of man, the writer recommended that it was wisest to view the matter in the light of the dietary habits of countries whose standard of living permits good health and well-being and to pay particular regard to the constancy in the proportion of calories derived from protein. The B.M.A. Committee was therefore reluctant to adopt any estimates of protein allowances expressed as grammes per day such as our American colleagues have set forth. They naturally felt that the figure of 1 g. protein per kg. of adult body weight so widely used gave a misleading impression of precision. The alternative to the problem accepted was to consider protein intakes in relation to calorie intakes.

During the First World War Starling used the phrase: 'Take care of the calories and the protein will take care of itself.' This as we have seen is sound advice for well-developed countries, but there are grave exceptions to Starling's generalisation when we consider less well-developed areas. For example, restriction of many African infants to their staple protein-poor cassava meal on weaning will almost certainly mean malnutrition. So it is important to emphasise that the analysis of diets that has supported Starling's dictum for the U.K. was concerned with intakes of mixed protein derived from typical dietaries, which have been found to be compatible with, on the whole, good health and well-being.

Proportion of Animal Protein

With rise in social class there is an increase in the proportion of protein eaten which is of animal origin (with milk showing the greatest proportional rise). During the period of food rationing this social distinction in the proportion of animal protein has been narrowed by a rise in the lower levels of the range. The pre-war lowest value was 36 per cent. of the total protein, the present lowest level is 48 per cent. The proportion in the highest social class of to-day, namely 60 per cent., is only a little below pre-war levels of a comparable class.

For pregnant women at the 6th–7th month the social gap in the proportion of animal protein in the diet is further narrowed, with a range by social class from 57 in the lowest to 62 per cent. in the highest, which is also an elevation of the whole range (Thomson, 1953).

Pre-war studies on children whose intake was not seriously affected by financial stringency showed proportions of animal protein from 65 to 75 per cent. of the total protein intake for younger children and 60 to 65 for older.

The rise in the consumption of animal protein which occurs wherever circumstances allow, is interesting when we consider that there is no proof that a proper mixture of plant proteins *per se* may not be as good for maintenance and even for growth and reproduction as proteins from animal sources. We have already noted that certain dietary essentials are associated with protein in animal foodstuffs, namely: fat soluble vitamins in varying measure in the associated fat; riboflavin and vitamin B_{12} among the water-soluble extractives, and that there are also evidences of possibly other accessory factors, for example Yudkin's (1952) recent observation of increased growth of children given a liver preparation. Further, there is no doubt that the attractive flavour associated with animal protein, freedom from associated indigestible cellulosic material and its wide range of culinary possibilities are very definitely contributing factors. Whipple's work would suggest that for the repair of tissues in convalescence a higher proportion of animal protein in the diet is advantageous.

In animal husbandry the farmer has learned to vary the proportions of protein and its quality in the diet according to the physiological need of the animal. We cannot, therefore, but be struck by the fact that adult man appears to eat more protein than he requires. Is the constancy of his protein intake as a proportion of the total calories an indication that he needs protein to form an optimal metabolisable mixture or is this constant ratio simply a reflection of a constant proportion of protein in all the cereals which bulk largely in his diet. Inspection shows that the latter can only be a partial answer. It rather looks as if man has tended, and still tends to take out of the common pot according to his energy needs, and through the ages the composition of this has also been adjusted to meet his maximal demands for growth and reproduction. If this be so, the safety margin is certainly wide for the adult man and non-pregnant woman who receive relatively too much protein in relation to their needs but without harm, and may indeed benefit from its specific dynamic action in cold weather. We realise that any other natural arrangement for the family would have been unlikely to survive, for no thought or care in respect of the total amount of protein need be taken in view of such a well-buffered system, provided the energy supply is sufficient. But to ensure a sufficiency of animal protein in face of its shortage, priorities or special feeding arrangements have come into being to meet what we judge, or should I say, guess,

to be needed to meet more efficiently the requirements of growth, reproduction and illness. The State has in part assumed the rôle of human husbandman and in some ways as individuals we do not like it for we sometimes display larger natural differences than do selected farm stock.

BIBLIOGRAPHY

AGRAWALA, I. P., DUNCAN, C. W. & HUFFMANN, C. F. (1953). *J. Nutrit.*, **49**, 29.

ALBANESE, A. A. (1950). *Protein and Amino-Acid Requirements of Mammals*. Academic Press Inc., New York, p. 116.

ALMQUIST, H. J. (1951). *Ann. Rev. Biochem.*, **20**, 305.

ALMQUIST, H. J. (1952). *Poultry Sci.*, **31**, 966.

ANDERSON, C. M., FRENCH, J. M., SAMMONS, H. G., FRAZER, A. C., GERRARD, J. W. & SMELLIE, J. M. (1952). *Lancet*, p. 836.

BLOCH, R. J., CANNON, P. R., WISSLER, R. W., STEFFEE, C. H., STAUBE, R. L., FRAZIER, L. E. & WOOLRIDGE, R. L. (1946). *Arch. Biochem.*, **10**, 295.

BOUSSINGAULT, J. B. (1836). *Ann. chim. phys.*, **63**, 225.

BROCK, J. F. & AUTRET, M. (1952). *Bull. Wld. Hlth. Org.*, **5**, 1.

CAMPBELL, P. N., WORK, T. S. & MELLANBY, E. (1951). *Biochem. J.*, **48**, 106.

CARPENTER, K. J., DUCKWORTH, J. & ELLINGER, G. M. (1953). In press.

CHALMERS, M. I., CUTHBERTSON, D. P. & SYNGE, R. L. M. (1953). *J. Agric. Sci.* In press.

CREMER, H. D., LANG, K., HUBBE, I. & KULIK, U. (1951). *Biochem. Z.*, **322**, 58.

CUTHBERTSON, D. P. (1940–41). *Nutr. Abs. Rev.*, **10**, 1.

CUTHBERTSON, D. P. (1950). *Brit. J. Nutrit.*, **4**, 232.

CUTHBERTSON, D. P. (1953). *Ann. Rev. Med.*, **4**, 135.

CUTHBERTSON, D. P & CHALMERS, M. I. (1950). *Biochem. J.*, **46**, xvii.

DEAN, R. F. A. (1952). *Brit. Med. J.*, **2**, 791.

DICKE, W. K. (1950). 'Coeliakie.' Utrecht, M.D. Thesis.

DUCKWORTH, J. (1952). *Chem. & Ind.*, No. 47, 1139, Nov. 22.

EVANS, R. E. (1952). *J. Agric. Sci.*, **42**, 438.

HEGSTED, D. M., BRIGGS, G. M., ELVEHJEM, C. A. & HART, E. B. (1941). *J. Biol. Chem.*, **140**, 191.

HIRSCHFELD, F. (1890). Virchow's *Arch. path. Anat.*, **121**, 501.

HOKIN, L. E. (1951). *Biochem. J.*, **50**, 216.

HOPKINS, F. G. & COLE, S. W. (1901). *J. Physiol.*, **27**, 418.

JOHNSON, R. E. & KARK, R. M. (1947). *Science*, **105**, 378.

LIENER, I. E. (1953). *J. Nutrit.*, **49**, 527.

MCDONALD, I. W. (1948a). 'Studies in the metabolism of sheep with special reference to the digestion of protein.' Dissertation—University of Cambridge.

MCDONALD, I. W. (1948b). *Biochem. J.*, **42**, 584.

MCDONALD, I. W. (1948c). *J. Physiol.*, **107**, 21P.

MCDONALD, I. W. (1952). *Biochem. J.*, **51**, 86.

MCNAUGHT, M. L., SMITH, J. A. B., HENRY, K. M. & KON, S. K. (1950). *Biochem. J.*, **46**, 32.

MAGENDIE, F. (1816). *Ann. chim. phys.*, **3**, 66.

MILNER, H. W. (1951). *J. Am. Oil. Chem Soc.*, **28**, 363.

MOORE, S. & STEIN, W. H. (1951). *J. Biol. Chem.*, **192**, 663.

MORAN, T. (1952). Report of the Research Assoc. of British Flour Millers. Gainsborough Press, St. Albans.

MORAN, T., PACE, J. & MCDERMOTT, E. E. (1953). *Nature*, **171**, 103.

MULDER, G. J. (1839). *J. prakt. Chem.*, **16**, 129.

OSBORNE, T. B. & MENDEL, L. B. (1912). *J. Biol. Chem.*, **12**, 473.

PEARSON, R. M. & SMITH, J. A. B. (1943). *Biochem. J.*, **37**, 153.

PETERS, F. N., CARROLL, R. W., BUNTING, W. R. & HENSLEY, G. W. Cited from Reports of the Quartermaster, Food and Container Institute for the Armed Forces, Chicago, Illinois, by Liener, I. E. (1950). *Trans. Amer. Assoc. Cereal Chem.*, **8**, 162.

ROSE, W. C. (1938). *Physiol. Rev.*, **18**, 109.

ROSE, W. C. (1949). *Fed. Proc.*, **8**, 546.

ROSE, W. C., JOHNSON, J. E. & HAINES, W. J. (1950). *J. biol. Chem.*, **182**, 541.

SCHOENHEIMER, R. (1946). *The Dynamic State of Body Constituents*. Harvard University Press, Cambridge, Mass.

SHERMAN, H. C. (1952). *Chemistry of Food and Nutrition*. 8th Edition, Macmillan, New York.

SYNGE, R. L. M. (1953). Personal communication.

THOMAS, K. (1909). *Arch. f. Physiol.*, 219.

THOMSON, A. M. (1953). Personal communication.

VOIT, C. (1867). *Z. Biol.*, **3**, 1.

WHIPPLE, G. H. (1942). *Amer. J. med. Sci.*, **203**, 477.

WIDDOWSON, E. M. (1947). Med. Res. Counc. Spec. Rep. Ser. No. 257. 'A Study of Individual Children's Diets.' H.M.S.O. London.

WILLCOCK, E. G. & HOPKINS, F. G. (1906). *J. Physiol.*, **35**, 88.

WILLIAMS, C. D. (1933). *Arch. Dis. Child.*, **8**, 423.

WOODMAN, H. E. & EVANS, R. E. (1951). *J. Agric. Sci.*, **41**, 102.

WOODS, E., BEESON, W. M. & BOLIN, D. W. (1943). *J. Nutrit.*, **26**, 327.

YUDKIN, J. (1952). *Brit. Med. J.*, **1**, 1388.

PSYCHOLOGICAL PROBLEMS IN THE RECOGNITION OF EXCELLENCE

Address by

PROF. D. W. HARDING

PRESIDENT OF SECTION J

THE British Association offers several kinds of opportunity in the relation of specialists to intelligent and informed laymen. The opportunity for the layman to hear something of the specialist's views is often taken. But the meeting offers an equally valuable opportunity for the specialist to consult with the layman, perhaps through informal discussions, perhaps implicitly by viewing his own preoccupations within the layman's perspective; and this is the opportunity I want to take.

To us as psychologists the most familiar aspect of the implicit consultation is the question what use, what practical applications, this or that branch of psychology may have. However, at this year's meeting in particular we remind ourselves that there is such a thing as science for its own sake, that the sciences form part of the total cultural wealth of our society, with a significance not to be fully estimated by the help they can give in the simpler day to day pursuits of health, safety and prosperity. This is a year when we can remember that intelligent laymen include not only men and women engaged in the practical affairs of daily life, and not only scientists working in the other disciplines, but also our colleagues in the humanities, and the artists and writers and thinkers who give our culture some of its distinctive qualities. They too provide standards and perspectives that may be relevant to the view we take of our work as psychologists. Implicit consultation with civilised people means not just telling them of psychological achievements or explaining the limitations of apparent achievements, but also reflecting on possible criticisms they might offer and disappointments they might feel, and considering their comments on problems not yet fully formulated, on work waiting to be done, and on the equipment of psychologists to carry it out.

One comment that they might make—and I think it would be fair—is that psychology on the whole is very much occupied with people of average ability and behaviour, or unsatisfactory deviations from the average, and has comparatively little to say about those who are outstanding for excellence of some kind. We can reply at once that there are good reasons for studying the average; for one thing most of us are average in most directions, and for another what is found out about the average often illuminates the exceptional, and vice versa. The only question is one of relative emphasis; and it seems to me true that we have taken comparatively little direct interest in valuably outstanding people. There are a few psychological studies of creative work and of invention, and Terman studied what he called geniuses, who were children of high intelligence quotient. Kretschmer wrote a book along popular lines on *Men of Genius*. A recent book edited by Paul Witty for the American Association for Gifted Children bears witness at least to a stirring of good intentions.

But how little it all amounts to compared with our abundant studies of the average, the subnormal, the abnormal and the delinquent. It is true that psychotherapists deal at times with exceptionally able and distinguished people, but then their attention is focused mainly on what is wrong with them. So too the writings of pyscho-analysts on outstanding men have been largely taken up with their

defects and oddities and the less gratifying psychological mechanisms involved in the production of their works. It seems fair to say that psychology has given more effective attention to the littleness of the great than to their greatness.

In more everyday experience we are from time to time puzzled at the later distinction of one child or student who seemed very ordinary and the comparative failure of another who showed brilliant promise, and in these unusual cases psychology can seldom give us much guidance in advance. In fact as psychologists we know rather little about the conditions of exceptionally valuable achievement ; or about the peculiar difficulties outstanding people meet in their work; or about the effects they have on their associates; or about the relations between them and the rest of us.

This last point may be specially important. Social progress seems very often to come about not by simultaneous advance on a broad front but through salients thrust out by unusual people and followed by a gradual consolidating movement by the group as a whole. The question is not only whether we can produce people capable of giving us a lead but also whether we can provide them with opportunities of doing their proper work and then take reasonably prompt advantage of whatever salients they thrust out. A great deal of social progress, in other words, depends upon the use that ordinary people can make of exceptional people.

The relation between ordinary people and the unusually able is a special case of the broader question of relations between people of different levels of ability or quality of mind, a question that lurks behind several familiar topics in social psychology. For example, leadership, whether we think of its pathological forms in twentieth century political movements or its acceptable forms in everyday working life, involves a special relation towards someone who is thought (or who claims) to have superior equipment of one kind or another. Education, too, in the broad sense implies a relation between people of different levels of ability. Competition refers to a testing of abilities in which, again, some people have greater success than others though all concerned have to find a way of continuing to live together. Much social psychology is concerned with questions of social status, the prestige of different occupations, and the hierarchy of jobs within an occupation ; and all these topics imply that certain people are recognised as being in some sense or in some direction superior to other people, with all sorts of consequences in their social relations. These are not matters of theory alone ; on the practical borderlines of psychology we can observe unmistakably how important these supposed differences of ability may be. In education, for instance, we have seen the anxious efforts of administrators to assure us that secondary modern and secondary grammar schools are equal in status, however different in their curriculum ; and we have seen the even more anxious efforts of parents to get their children into grammar schools. One is reminded of George Orwell's *Animal Farm* where ' all animals are equal but some animals are more equal than others.' It looks as though the time may be ripe for a more direct examination of this broad problem.

Its seriousness lies in the waste of human energy and human capacity that may occur through friction between people who have not adjusted themselves satisfactorily to mutual differences of ability. The invitation to observe and recognise the superior achievement or capability of someone else may sometimes elicit generous and willing admiration ; but at other times it produces jealousy, betrayed by a grudging tone in the expression of admiration, an eager fault-finding eye in order to disparage where disparagement is possible, dwarfing the superior person by comparing him with someone still more gifted, or attempting to explain the superiority in other terms than the merits of its possessor—' Well, of course, look at the advantages he's had.' Nor does jealousy stop at disparagement and grudged praise : it sometimes leads to the active obstruction of the abler person's further achievement. It may, for instance, poison the relations between an older man and a young colleague who is destined to surpass him. William Blake expressed these possibilities vividly when he exclaimed, ' Is not Merit in one a Cause of

Envy in another, and Serenity and Happiness and Beauty a Cause of Malevolence ? ' (quoted by Mona Wilson, *Life of William Blake*). This is one-sided, no doubt, but there is equally no doubt that the ugly side of social relations between the meritorious and the less meritorious does exist.

Happier relations are also possible, and one of the great satisfactions of social existence is to be found in the spontaneous and generous recognition of achievement that outdoes one's own, in the relation for instance of followers to a leader or of the audience to a performer. But we know surprisingly little of the factors making for a generous response to superior achievement, or even of the conditions that allow excellence to be recognised when it presents itself.

Dramatic instances of the waste that may occur through a failure to recognise great capacities are of course familiar enough in the lives of inventors and reformers, prodigies of the arts and gifted people in general. And yet in all probability a much greater volume of waste occurs through the obstruction or neglect suffered by large numbers of people of more moderate capacity. An instance of someone rather below the rank of the very great but still gifted enough to enrich his profession is to be found in Sir Henry Thompson, the nineteenth century surgeon ; and his recent biographer, Zachary Cope (1951), records how very nearly the obstruction of a well-intentioned father prevented him from having any career at all. Thompson's fascination with surgery showed itself at an early age. But his father was a dissenter of Calvinist inclinations and regarded the study of medicine as inimical to a firm belief in divine revelation. He was also a grocer, draper and tallow chandler in a small market town and wanted his son to maintain the business. He insisted, therefore, that he should come into the business, in spite of Thompson's plea to study medicine ; and from the age of fifteen to twenty-seven Thompson worked in this uncongenial occupation with reluctant obedience and deteriorating health. At last, in face of a serious breakdown in his son's health which necessitated three months' rest, and under the persuasion of the physician, the father did consent to Thompson's entering on a medical career, in which he rose steadily, not merely to eminence and a baronetcy but to genuine usefulness. The father was rapidly reconciled to his son's new career once he had consented to the change. But Thompson, until the end of his life, kept the letter in which his father refused his first request, at the age of fifteen, to study medicine, and he records that the twelve years from fifteen to twenty-seven were a time of ' over-ruled wishes, thwarted aims, uncongenial occupations and associations, much wasted force, great anxiety, distress of mind, dejection of spirits and seriously impaired health.'

We know the history of Thompson because the obstructions placed in the way of his great ability were not fatal, but for each case of relatively unsuccessful obstruction of ability there must be many, unknown, in which talents and unusual capacities are entirely wasted and their potential value never glimpsed.

Because this kind of thing is found so frequently in biographies of the eminent, and because the successful strangling of talent precludes *ipso facto* the biographical record of it, we may be tempted to take it for granted and perhaps comfort ourselves by regarding the obstacles as a useful toughening for young talent. We may equally take for granted the obstacles met with in later life by innovators whom the conventional members of their profession oppose, men like Lister or Freud. Here too we read only about the people who broke through the barriers ; it seems conceivable that there are many more whose possible contribution is successfully burked and we go happily on knowing nothing of what we have missed. It would take a very determined optimist to maintain that in human society as it is now the best possible use is made of available talent and potential excellence.

The assessment by parents of their children's capacities is obviously of vital importance, but also hedged about with difficulty. On the one hand there is the temptation to suppose that all your geese are swans ; on the other the equally significant story of the ugly duckling. Henry James points to a real possibility

in *The Bostonians* when he depicts a commonplace mother's difficulty in grasping the distinction of her unusual daughter : ' She was proud of Verena's brightness, and of her special talent ; but the commonness of her own surface was a non-conductor of the girl's quality.' Most parents would feel, unless they were supremely self-satisfied, that one of their greatest good fortunes would be to have a child whose capacities would outdo their own. It seems to be assumed that if that good fortune should occur they would easily recognise it and know by nature how to make the best of the situation. I doubt it. And I suspect that if parents need advice on such things as punishment and children's fears and backwardness and sexual enlightenment, there is also a case for advice on noticing and responding to qualities and capacities beyond their own.

Of course, the same problem faces teachers. It is quite evident on general grounds that most teachers will be dealing with an appreciable percentage of children who are potentially abler than themselves. Detailed evidence on the point is available from studies made of the intelligence test scores of university graduates in training as teachers, for instance by Professor Rex Knight (1933), and in Liverpool more recently by Mr. Tozer ; and taken with what we know of the distribution of intelligence quotients in the child population this evidence would allow a rough estimate of the proportion of school children whose intellectual capacity will eventually be greater than their teacher's. It is certainly not a negligible proportion, and some few children will have minds of markedly greater potentiality than their teachers. We may say, and quite soundly, that the teacher's advantages of special training, acquired information, practised habits of work and general experience can enable him to be of the utmost service to minds better endowed than his own. Yet it would be foolish to pretend that no difficulties or dangers exist, and to assume that the average teacher can always distinguish the child of exceptional capacity from the ordinarily able one and can give the potentially outstanding child the stimulus he needs. The fact that the child with very high intellectual endowments is not always easy for parents and teachers to handle is evident in Professor Vernon's finding, from the analysis of data from a child guidance clinic, that children of very superior intelligence were more liable to personality difficulties than the moderately superior. But of course many potentially outstanding children who never break down still fail to develop their capacities, or do so only in later life when they have left school.

Educational experience in Sweden, recently reported by John Elmgren, bears out the same point. Dr. Elmgren writes :

> ' On account of their more complex aptitude structure, the extremely intelligent pupils often have difficulty in adjusting themselves to ordinary school conditions, which must of course for organisational reasons be designed for the needs of the average intelligence. From the point of view of character, this entails certain risks for just those who are most gifted intellectually, who do not need to make any particular effort to keep up with their school work, and who often grow apathetic because of insufficient stimulus to their interest. In individual cases the result can be the formation of bad habits, which psychologically can be disastrous enough for the pupil's future development, especially if one remembers that extreme intellectual gifts are often combined with temperamental instability, neurotic characteristics, or environmental difficulties of different kinds.'

I have been speaking of the outstanding child in terms of intelligent quotient, but that is a small part of what I have in mind. I am thinking, of course, of excellence in any direction, including qualities of intellect not measured by intelligence tests, and special aptitudes, and qualities of personality.

I want to emphasise that I see no reason why a teacher should not win the respect of pupils far abler than himself and help them to realise their potentialities, but it may be a part of the art of teaching that involves more special difficulties than are always grasped. It seems to me an important mark of successful teaching that a pupil can look back gratefully and with appreciation to teachers whom he has far

out-distanced. The same applies to leaders generally and people in any senior position; it is one test of their capacity as leaders whether they gain and retain the respect of abler people who have at one time been junior to them.

In approaching this area of social psychology, the initial step must be to resolve the broad and rather vague problem, as it first presents itself, into smaller questions that can be formulated more precisely and can eventually be studied in a disciplined way. We have to make a transition from the very broad question that the layman might ask to the more detailed problems that the psychologist would have to examine in order to find an answer. I want to suggest a few questions, perhaps half-way towards the technical questions that might finally have to be asked, and to group them roughly under two headings: those to do with the perception of excellence, and those to do with our response to it once it has been perceived. And I have to remind myself that excellence is of course a matter of degree, and the question is how we behave not only in face of superlative excellence but also when we meet the less striking but still vitally important differences of ability in everyday life.

Behind the particular problems that face parents and teachers in dealing with the exceptionally able child, there lurks the general question of how and in what circumstances we manage even to perceive achievements and qualities as being superior to our own. A passage in one of Jane Austen's novels puts this very difficult psychological question in quite simple terms. It occurs after Mr. Elton has been hopefully presumptuous enough to propose to Emma, and as she recovers herself late that evening she reflects, ' Perhaps it was not fair to expect him to feel how very much he was her inferior in talent, and all the elegancies of mind. The very want of such equality might prevent his perception of it. . . .'

It seems evident that the more intangible the quality or achievement, the more lacking in overt expression, the more difficult it will be for us to recognise other people's superiority. Sports and games have the great advantage here of being overt performances, many of them with almost inescapable criteria of excellence. And with this initial advantage the institutions and conventions of sport have established a comparatively good relation between winners and losers, and between people of varying degrees of ability. We need not over-idealise sport in this connection or suppose that it presents a picture of undisturbed harmony, but few people would deny that it is an area of activity in which competition has been brought within a satisfactory framework of co-operation and made consistent with good personal relations between people of widely contrasting levels of ability.

Other quite overt activities, such as dancing, riding, car driving, lack the kind of objectively measurable results that cannot be argued away. Then the question whether we can assess our own performance objectively depends very much on the extent to which our self-esteem is staked on it ; many men would cheerfully admit that they were poor dancers, but few motorists—not 50 per cent. I feel sure—believe that the man next door is a better driver, any more than their wives believe that the woman next door manages her housekeeping money better.

When it comes to the intangible qualities an objective assessment of ourselves is particularly difficult. G. W. Allport (1938) reports an experiment in which students assessed themselves for sense of humour and insight, and he found that about 95 per cent. considered themselves average or above in these two qualities. A deficiency in certain other qualities is probably unnoticed by the man who lacks them because he never thinks about those qualities. The desiccated person would perhaps not rate himself high for creativeness if he were asked about it but in the ordinary way he would never think of it, and if he met a creative person he would notice only the disorderly impulsiveness and the unfortunate lack of administrative capacity. This kind of thing can be regarded as part of the process that experimental psychologists have studied in a narrower form under the heading of ' perceptual defence.'

However, it would be wrong to suppose that we always overrate ourselves on the intangible qualities that we and our

culture prize ; David Riesman (1950) has shown that many Americans, accepting the high value that their culture places on good temper and affability, are prone to accuse themselves privately of being more quick-tempered than they really are. We should have to take account of the possibility of irrationally high standards, with an accompanying sense of guilt, if we were to attempt any adequate account of objectivity in self-assessment.

Further, we should need to consider our distance in time from the behaviour we were assessing, for it certainly seems as if we can compare ourselves more accurately with our companions when we look back at an earlier period of our life than we were able to at the time. And these are no doubt only a few of the factors that affect our self-assessment and our recognition of the relative merits of others.

In considering how we perceive other people's superiority we are handicapped by knowing very little about the process of making and perceiving an advance in our own behaviour and standards. We can all of us look back at earlier periods in our life and see that by our present standards we are better now in some direction than we once were ; perhaps our insight into other people has increased, our responsiveness to literature or the arts has developed, or we may have become less conceited. The ethical and aesthetic problems that lurk here, I gladly leave to those who are qualified to discuss them ; there are questions enough of a psychological kind arising out of the fact that at any rate we believe we have advanced. We know little about the ways in which a man comes to adopt new standards, or what he is likely to think of the old ones and of people who cling to them. Does he feel a sympathetic understanding of people who are still at a stage that he has left, or is he like children who are especially intolerant of others for showing the inferiorities that they themselves have only just overcome ?

It seems very possible that what we feel about the stages we have already gone through may influence our view of the possibility that there are advances still to be made, and this in turn may affect our attitude to people whose standards are different from our own and possibly better. The openness of mind that permits us to believe effectively that some people may be better than ourselves is possibly connected with what we went through while we ourselves became better than we previously were.

These are only some of the questions we should have to ask in order to understand the conditions under which we can perceive excellence and human quality surpassing our own. Hints and materials for the answers are scattered over many fields of psychology, but our chief finding at present must be the extent of our ignorance and uncertainty in this area of social relations.

The sheer failure to perceive the superiority of other people, which accounts for some waste of human resources, shades off imperceptibly into a reluctance to give generous acknowledgement to superiorities that we perfectly well perceive. At one extreme there may be complete blindness to excellence, and at the other clear awareness of it accompanied by jealous hostility, but in between there is a much bigger twilight region of evasion and rationalising, in which the fear of what may disturb our self-esteem makes us to some extent genuinely unaware of it. However, I am not forgetting that a very much more satisfactory attitude is also to be found. A generous interest in the achievement of others, based on the knowledge of having fully developed one's own special powers, is illustrated by the attitude of G. H. Hardy, the distinguished mathematician. He writes :

> ' If a man has any genuine talent, he should be ready to make almost any sacrifice in order to cultivate it to the full. This view was endorsed by Dr. Johnson—" When I told him I had been to see [his namesake] Johnson ride upon three horses, he said ' Such a man, sir, should be encouraged, for his performances show the extent of the human powers . . . ' "—and similarly he would have applauded mountain climbers, Channel swimmers, and blindfold chess-players. For my own part, I am entirely in sympathy with all such attempts at remarkable achievement.

I feel some sympathy even with conjurors and ventriloquists ; and when Alekhine and Bradman set out to beat records, I am quite bitterly disappointed if they fail. And here both Dr. Johnson and I find ourselves in agreement with the public. As W. J. Turner has said so truly, it is only the " highbrows " (in the unpleasant sense) who do not admire the " real swells ".'

Although this view of G. H. Hardy's seems worth quoting, I want to guard against an easy misunderstanding. The problem is not one of recognising this or that man as outstanding, of identifying the so-called ' great man ' and establishing a satisfactory relation with him. The crucial and more difficult task is that of recognising the excellence of ordinary people, particularly the unexpected excellence and the disturbingly unequal excellence. The greatest volume of waste must occur among fairly ordinary people. In fact the public recognition of a ' great man,' a member of a special category of ' great men,' is relatively easy ; it puts the man apart and spares us the need to compare his achievement with our own. It is also highly artificial and unreal, for it evades the complexity of the facts, which are usually that a man's greatness occurs in streaks and patches, interspersed with mediocrity, inadequacies and even littleness. The rather difficult discrimination consequently required was notoriously evaded in the nineteenth century with its erection of contemporaries into great men and great women. That inevitably involved the veiling and sometimes the total concealment of the less creditable features of the great ; and, naturally enough, later generations have been dismayed, or gratified, by corrective revelations.

In casting about for an explanation of the nineteenth century readiness to slip from the recognition of great qualities in a man into the fiction that he was altogether great and sacrosanct, we are bound to remember that it was also a period of parent worship. There were strong pressures on well-brought-up people from childhood onwards to organise their feelings around a belief in good and powerful figures. Criticism and discriminating appraisal of them generated too much uneasiness or guilt to be readily attempted. I believe that any thorough insight into people's attitudes to greatness and excellent achievement would have to take account of their relations as children to their parents and other grown-ups. For example the person who has never freed himself from an exaggerated view of parental superiorities may find it difficult to admit any claim to greatness made on behalf of his contemporaries ; the best of them is clearly so far below his unconscious fantasy of his father or mother. And on the other hand the man who had to make too violent an effort to free himself from the parental myth may have a continuing compulsion to debunk all claims to greatness. In considering this and similar possibilities, such as the influence of early sibling jealousy on later attitudes, we should look to psychopathology for explanations of some people's reluctance to give generous appreciation to high achievement.

It would be misleading, however, to speak as though a good relation between people of differing ability depended only on the attitude of the less able. Both sides must contribute. If the one is not to be jealous and resentful the other must equally not be patronising or disparaging.

If the less able person is to feel self-respect and security in spite of not excelling he has to be sustained by two convictions. The first is that what he can do, his own proper function in the shared task or the common life, is respected by the abler person even if it makes only a small contribution. The second is that in some sense his equality as a person is recognised, and that the measuring rod against which he falls short implies no judgment of his worth as a living being.

This question of personal equality seems to me crucial in the relation between the ordinary and the excellent, and it has not received much attention from psychologists. No doubt they have left it to philosophers, for its philosophical aspects are formidably prominent. But it must be a matter of concern to psychologists, especially to democratic psychologists who believe that all men are born equal and then spend a great part of their time demonstrating and measuring innate inequalities between them. The philosophical and political questions are not

our concern here. In the psychological context the notion of equality implies that although one man may do better than another at an intelligence test, although he may do better at every available test and in every situation of real life, still the living activity of each man is equally important to him. I may be better than a mental defective at all sorts of intellectual function and I may be more useful socially for this purpose and that, perhaps for every purpose, except for the supreme purpose of living my own life, and in that I can do no better than he can. His satisfactions are as important to him as mine are to me; whatever disappointments he can feel are not more or less disappointing than mine. It would be as foolish for me to feel personally superior to him, however functionally superior I might be, as it would be for a tiger or a liver-fluke to feel superior to me because it can do things that I can't. As Professor Mace (1953) has admirably said, 'Each kind of creature endeavours to persist in its own being and in so persisting to enjoy its own distinctive quality of life,' and I would add that it is in doing that that it possesses equality with every other creature.

This is all rather abstract. But a great many thoroughly practical people understand it intuitively and express it in their way of controlling their subordinates. They achieve a state of affairs in which they can direct the work of others, keep it up to standard, criticise when necessary, and maintain discipline, but still leave their subordinates' self-respect intact and produce in them no sense of personal belittlement. The way in which they achieve it is not our present concern, beyond the fact that it is not done by calculated devices for 'handling people'—to use an appalling and revealing idiom.

The view that one criterion of good leadership is the subordinate's sense of being respected represents a principle that can be applied more generally to the relation between people of different levels of ability. It seems essential that those of average and less than average ability should be genuinely respected by abler people for the contribution they can make. The psychological achievement and the social importance of contributing cheerfully what is in one's power even when it is obviously not very distinguished, is something that should not be underrated; and that may be one of the most important social lessons that the abler man or woman has to learn.

How are we to attack some of these problems? Shall we approach them, as psychologists so often approach their problems, by studying people whose status is in some way less than our own—children, students, lower-paid industrial workers, other ranks in the forces, primitive peoples and neurotic patients? I think it may have been a mistake for psychologists to do so much of their work with subjects over whom they had some advantage, and in this particular problem it would be specially limiting.

No doubt we could gain something from the study of such people; and their behaviour might by inference illuminate the behaviour of abler, civilised and normal adults. But it would be in keeping with the English empirical tradition not to rely entirely on an extension of the findings by inference but actually to study the problems among the people with whom we are ultimately concerned. Unfortunately this suggestion will strike many contemporary psychologists as not merely revolutionary but flippantly impracticable and irresponsible. Only a tiny proportion of reputable psychologists would think it their business to discover all they could about the thinking processes, the development of interest, the changes of outlook and the social relations of people whose ability and distinction equalled their own or excelled it, the inventors, writers, mathematicians, religious thinkers, responsible administrators. David Riesman (1952) has commented recently on one aspect of this selective attention. He writes,

> '. . . few social scientists have interested themselves in the autonomous and happy person; they have been more preoccupied with misery and lower-level adjustment. It is, in many ways, easier to recognise and study the latter; moreover, to do so is frequently gratifying to the observer who is enabled to patronise what he studies and to avoid the discomfort of realising that it may hold lessons for him personally.'

Apart from the problem of humility to which Riesman alludes, there are, as he hints, difficulties in the way of the psychologist's recognising and studying the activities of abler people. He may not be equipped to do it. In the literary field, for instance, some of the most interesting psychological work has been done by non-psychologists, such as Livingstone Lowes in his study of Coleridge's transmutation of his raw materials. Again, several scrupulous biographical studies that have recently been produced by American scholars are very close to the spirit of scientific psychology in their disciplined and well-documented organisation of factual materials, although they wisely avoid psychological interpretations. I need hardly say that I am not referring to the studies that appeared between the wars and explained the careers of notable figures in terms of their complexes or endocrine glands. I have in mind rather such books as A. M. Terhune's study of Edward FitzGerald, and Arthur Mizener's life of F. Scott Fitzgerald, the novelist. At present it is not psychologists who have shown most capacity for the sensitive and accurate handling of materials like these.

A recent paper in the *Journal of Abnormal and Social Psychology* is relevant here. It was admirably brief, consisting largely in a quotation from *The Scarlet Letter* by Nathaniel Hawthorne, a passage in which Hawthorne describes the relation between a sensitive physician and his patient. As the author of the paper, Howard Brand, points out, the passage states with great accuracy and understanding what we now describe as the psychotherapeutic relation and he finds it a startling comment on the present status of psychotherapy. He asks in a series of rhetorical questions whether psychology is able to say very much more on this topic than Hawthorne expressed over a hundred years ago, and he ends with the comment that 'It is sobering to observe that our skills and our techniques are matched by the acumen of a sensitive writer on human relations.'

My reaction to this passage from Hawthorne was in one way different from Dr. Brand's, although I agreed entirely with his high appreciation of it. What startled me was the surprise expressed by a psychologist at finding that a writer of the calibre of Hawthorne could go at least as far as the ordinary run of psychologists in subtlety of understanding and insight. It gave me a sharp reminder of the fact that very many psychologists simply do not realise that fact, or show no signs in their work of realising it. If the insight of Hawthorne startles a psychologist what will he feel when he reads Henry James? To mention only a broad and public theme, the understanding of national differences scattered throughout James's work is so subtle and probably so accurate that I should feel a little uneasy if I were asked to demonstrate how much had been added to it by social psychology.

But I should be leaving a very false impression if I implied that great writers or other distinguished observers even at their best could take the place of psychological investigators and scientific techniques. The situation is much more complex than that. To go back to the note about Hawthorne, I should not accept for a moment the implication of Dr. Brand's rhetorical questions that psychology has added little to the understanding Hawthorne could offer us of the psychotherapeutic relation. Hawthorne's fantasy sketched in a possibility and no doubt recorded in heightened form some actual observation of a man who had a good clinical attitude. I should say that modern psychotherapy can give us (in concepts such as transference and resistance) a very much more detailed and intelligible account of the processes that occur in the therapeutic relation. Moreover it can bring within the reach of many ordinary people with an appropriate training the actual performance of the therapeutic role, or some approximation to it, that Hawthorne could only create as an imagined possibility for a very unusual doctor in a novel.

Again, the insights of any subtle mind—such as Henry James on national differences—are liable to be countered by conflicting insights from some other mind; and who is to choose between rival intuitions? What brought me to psychology was the hope that a scientific discipline could clear the ground in such a case and make some small advances towards greater probability in judging the validity of seemingly subtle or profound

insights into complex topics. Psychology has given us, beyond doubt, very much more solid ground than we ever had before ; it has crept along, necessarily in the rear of the best work of the finest minds, checking and confirming and consolidating wherever it could, sometimes raising profitable doubts, sometimes giving significant stimulation to good minds working in other fields. This seems to me a thoroughly valuable programme for a science which is bound to be dealing, for much of its time, with the subject matter of the arts and humanities, of occupational work, of politics, of personal relations and of civilised life generally.

Two dangers have to be noted. One is that we may be tempted to work with disproportionate intensity on the small and manageable problems, because they come within range of existing scientific techniques. Large problems must be subdivided ; detailed work must go on, and it sometimes produces unforeseeably big results. But there is a danger of becoming niggling and losing sight of the bigger concerns to which we had originally hoped to contribute.

When, on the other hand, we do approach in a scientific spirit some of the more complex problems, we may, as I have suggested, meet a second danger. We are often dealing then with the interests of exceptionally able minds, sometimes of very sensitive minds, which have developed much further in a special direction than we can go with any real competence. The danger then—and it has not been entirely avoided in some psychological excursions into the arts and politics—is of being crude or naïve. That danger can be avoided by the right sort of relation between the psychologist and the people with special and often greater capabilities. But this implies that the psychologist should be able to perceive the special superiority of the minds he is dealing with and that he and they should be capable of entering into a mutually useful relation in spite of disparity in ability.

And so I find myself back by a different route at my broad problem, of the perception of excellence and the relation we establish with those whose achievement stands at a higher level than our own. I have suggested that this is a psychological problem ramifying widely through social life and at times producing difficulties that rob us of some of the benefits we might gain from our abler people, whether additions to our material well-being or extensions of the less tangible resources and opportunities of our culture. It is a problem about which psychology already has a great many relevant facts and principles ; but the time may have come to focus attention on it rather more explicitly. The psychologist could go only a little way into the problem without himself having to deal with minds that might well be better than his own. That possibility demands thought, and I feel no doubt that much of our thinking about it is rightly done in the company, such as we have at a meeting like this, of informed colleagues and friends who are not themselves psychologists.

References

Allport, G. W. (1938). *Personality.* London.

Brand, Howard (1952). 'Hawthorne on the Therapeutic Role,' *J. Abn. & Soc. Psychol.*, **47**, 4.

Cope, Zachary (1951). *The Versatile Victorian: being the life of Sir Henry Thompson, Bt.*, 1820–1904. London.

Elingren, John (1952). *School and Psychology: a report on the research work of the 1946 School Commission.* Stockholm.

Hardy, G. H. (1940). *A Mathematician's Apology.* Cambridge.

Knight, Rex (1933). *Intelligence and Intelligence Tests.* London.

Kretschmer, Ernst (1931). *The Psychology of Men of Genius.* London.

Lowes, J. Livingstone (1927). *The Road to Xanadu.* London.

Mace, C. A. (1953). 'Homeostasis, Needs and Values.' *Brit. J. Psychol.* **44**, 3.

Mizener, Arthur (1951). *The Far Side of Paradise.* Boston.

Riesman, David (1950). *The Lonely Crowd.* New Haven.

Riesman, David (1952). *Faces in the Crowd.* New Haven.

Terhune, A. M. (1947). *The Life of Edward FitzGerald.* London.

Terman, L. M. (1926–1930). *Genetic Studies of Genius.* Stanford University Press.

Vernon, P. E. (1937). 'A Study of the Norms and Validity of certain Mental Tests at a Child Guidance Clinic.' *J. Educ. Psychol.* **7**.

Wilson, Mona (1927). *The Life of William Blake.* London.

Witty, Paul, ed. (1951). *The Gifted Child.* Boston.

THE EVOLUTION OF THE OVULE IN THE PTERIDOSPERMS

Address by

PROF. JOHN WALTON

PRESIDENT OF SECTION K

THE dominant plants of the present time, Gymnosperms (conifers etc.) and Angiosperms (flowering plants in the popular sense), reproduce by means of seeds, and the history of the plant world as revealed by the fossil record suggests that propagation by means of seed had given these plants a distinct advantage over plants which depended on the method of reproduction by spores and exposed gametophytes employed by ferns and their allies. The seed habit is particularly well suited to life on land. The Pteridophytes, on the other hand, have delicate gametophytes which depend on the presence of free water at the critical moment when fertilisation is effected by motile gametes. Compared with the seedling the young sporophyte has a poor start in life and, having no concentrated food reserve provided by its parent, usually takes a considerable time to develop to an appreciable size. It is therefore subject to severe competition with plants which develop from seeds containing a food supply which enables the seedling to develop its photosynthetic area rapidly. The retention on the parent plant of the spore destined to produce the female gametophyte protected by the sporangium and integuments has made possible the development of refined methods of ensuring cross or self pollination. By such methods the close approximation of male and female gametophytes in an enclosure cut off from fluctuations in the external environment is assured. The most important characteristic of a seed, or its younger stage the ovule, is that the delicate female gametophyte is securely protected inside the ovule and the critical process of fertilisation is carried out inside the well-covered ovule without reliance on an external water supply over which the plant has no control.

Of the other characteristics of modern seeds the presence of a reserve of food material, either in the endosperm or embryo, is general but there is not necessarily a thick resistance testa and while some seeds can undergo a long period of dormancy, many must be given the opportunities to germinate at once on liberation or die.

In vascular plants a resistant resting structure may occur at a different stage in the reproductive process. In the Salviniaceae and Marsiliaceae the sporangia are enclosed in the sporocarp. This, when detached from the parent plant, may remain in dry dormant condition for long periods but, as in all other Pteridophytes, fertilisation cannot take place until free water is available.

The sporocarp, although it is morphologically comparable to a seed, having sporangia enclosed in a foliar integument, does not fulfil what I consider the fundamental requirement of ensuring that fertilisation occurs in an enclosure cut off from external influences.

Fertilisation probably occurs in all known ovules before liberation, but the development of the embryo from the zygote may occur after the seed is shed. In Ginkgo the embryo may develop before or after liberation.

With the exception of a few special cases the modern ovule compared with the earliest known fossil examples is relatively simple in construction. This difference is undoubtedly related to the fact that the ovules of Palaeozoic plants and in particular the Pteridosperms, an extinct group of Gymnosperms, were borne on the leaves

or fronds of the plant and were fully exposed whereas in the majority of modern Gymnosperms and Angiosperms the ovules are protected by cone scales or enclosed completely in ovaries.

The modern ovule has as a rule a covering made up of two sheathing integuments surrounding the sporangium with its single functional spore. The funicle or stalk of the ovule has a vascular strand but generally no vascular supply is present in the integuments. There are, however, some interesting exceptions to this in some of the more primitive orders of Angiosperms. The nucellus or sporangium consists of a relatively undifferentiated rounded mass of parenchyma. In contrast to this the ovules of the extinct Pteridosperms which afford us the earliest structurally preserved examples of ovules have very often two integumentary sheaths both of which are furnished with a well-developed vascular system and the free apex of the nucellus has a most elaborate structure.

A study of these early seeds leads one to the conclusion that there have been several important trends in ovular evolution. We may venture to suppose that the ovule in all the seed plants has evolved from a sporangium or fertile telome which retained a single functional spore and that the sporangium in the Pteridosperms at least has become invested with successive sheaths of sterile parts of the frond. Gradually the function of pollen capture was taken over by the surrounding integuments which closed in over the sporangium and formed a micropyle which became responsible for holding the pollination droplet. In the mesozoic Pteridosperms, e.g. Caytoniales, we find the ovules almost completely enclosed by a part of the frond, the cupule, which has a stigma-like flap at the entrance. Finally, in the Angiosperms the ovules are completely imprisoned and pollen cannot reach them. The ovules have no mechanism for capturing pollen and in many plants the micropyle performs no known function. The capture of pollen is effected by the carpels and being completely enclosed there is rarely a development of vascular tissues in the ovules.

The complexity of nucellar structure and the extent of vascularisation of the integuments have evidently diminished with the increase of protective coverings. Some of the earliest known seeds were probably dispersed by wind, and it is possible that those in which a fleshy integument or cupule developed, such as the Trigonocarpales and Caytoniales, may have been dispersed by animal agency.

The Pteridosperms, an extinct group of vascular plants provide us with examples of the earliest types of plant known to have borne ovules. The existence of this class of vascular plant was recognised in 1899 by Potonié, and named by him the Cycadofilices. At that date all that was known about the group was the structure of their vegetative parts—stem, leaf and root. The early investigators considered that in the structure of these organs there was apparent a mixture of fern and cycad characters.

In 1903 Oliver and Scott established the connection between *Lagenostoma Lomaxi*, an ovule, and the fern-like frond *Sphenopteris hoeninghausi*, hence with the stem *Lyginopteris oldhamia*, which bore this type of frond and proposed that the group should be named the Pteridospermae—a name now generally accepted and used.

The earliest examples of ovules hitherto discovered are of Lower Carboniferous age and they are ovules of Pteridosperms, judging from their structural resemblance to ovules definitely known to be those of Pteridosperms. No undoubted ovule or seed has been found in earlier geological formations. The claims that have been made that certain Devonian fossils are of seeds or ovules have not so far been substantiated, and while it is more than likely that ovules existed in the Devonian, no evidence of any scientific value exists to support any such claim beyond the occurrence of gymnospermous types of woody stems in strata as early as the Middle Devonian.

Several palaeobotanists have remarked that no Pteridosperm ovule has been found with a recognisable embryo although quite a considerable number have been found with a well-preserved female prothallus. Some botanists maintain that the Pteridosperms were therefore not seed plants as no embryo was formed while the ovule was still attached to the parent plant. Emberger has therefore proposed that the Pteridosperms and Cordaitales should be

placed in a group, separated from the other seed plants, which he calls the Prephanerogams. This division into two major classes of what has hitherto been regarded as the seed plants, would appear to be somewhat arbitrary, including as it does two phyla distinct at least as far back as the Lower Carboniferous and employing a time difference in the development of a zygote or embryo as the most important diagnostic feature; moreover no agreement has yet been reached as to the precise definition of a seed.

There is no doubt, judging from the structure of Pteridosperm ovules and the presence in them in some instances of considerable members of pollen grains of a single type in the pollen chamber, that pollination occurred on the parent plant. Apart from the absence of an embryo they appear to be mature structures and in most instances have a considerable development of hard stony tissue in their integument. It is therefore difficult to imagine germination occurring without the disruptive action of the growth of a substantial embryo. The well-developed testa with its stony layer also suggests that the ovule or seed may have existed in a dormant state for a considerable period before germination occurred.

Arnold considers that the time of development of the embryo during the time that elapsed between pollination and the beginning of a period of dormancy is unimportant and that these Pteridosperm ovules did eventually become seeds in the generally accepted sense. There seems therefore little justification for excluding the Pteridosperms and Cordaitales from the seed plants or Phanerogams.

In most living Gymnosperms the embryo is completely developed before the seed is liberated, but as in Ginkgo the development of the zygote may occur after liberation.

Ovules or seeds in the fossil state present us with special problems. Mention has already been made of the fact that hitherto no examples of Pteridosperm ovules containing an embryo have been found. Examples which have been found attached to fronds are all compressions and it is impossible to determine whether they had embryos or not. Of the structurally preserved examples a few have been found with gametophytes but none have embryos. It is possible that pollination carried out by the agency of wind was uncertain and that many infertile ovules resulted. The majority of the fertile seeds must surely have germinated disrupting the testas and we would therefore not expect to find many containing embryos in a state of petrifaction. An apple tree may shed a considerably larger number of abortive than fertile fruits and a palaeobotanist of the future might perhaps reasonably deduce from the fossil remains of apples that seed development occurred on the ground.

The ovules attributed to the Palaeozoic Pteridosperms may be divided into two major groups—the Lagenostomales and the Trigonocarpales. The former contains Lower and Upper Carboniferous species while the latter consists entirely of Upper Carboniferous species. The Lagenostomales exhibit a greater range of structure with perhaps more primitive characteristics and will be considered first.

The Lagenostomales

The late Professor W. T. Gordon has given us a detailed description of what is probably one of the oldest known types of seed, *Salpingostoma dazu* (Fig. 1 on page 226), from the Lower Carboniferous of the Lothians. The same type of seed also occurs in Calathospermum a large cupule of the same geological age which contained a large number of ovules of the Salpingostoma type. The cupule here may perhaps be regarded as a common outer integumentary investment of several ovules. The ovule has a most elaborate nucellar apex more complex than those of Upper Carboniferous Pteridosperm ovules. The lower part of the nucellus contained the single spore of which the cutinised membrane is the only part preserved. The upper part is differentiated into a dome-shaped pollen chamber which at the apex is prolonged into a long trumpet-shaped extension, called by Gordon the salpynx, down which the pollen passed to the pollen chamber. The nucellus is surrounded by six terete processes, each containing a vascular strand, which are fused to the lower part of the nucellus but become free at about the level of the pollen

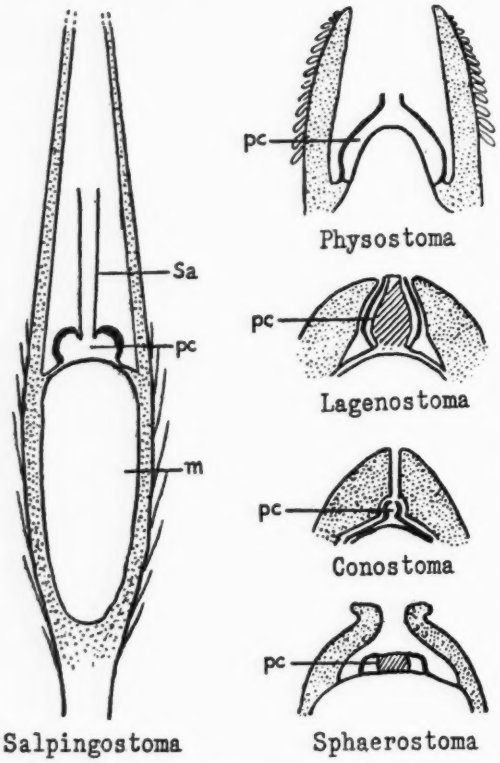


Fig. 1.—Diagrammatic representations of the nucellar apex and micropyle of the ovules of some Lagenostomales.

m, megaspore; pc, pollen chamber; sa, salpynx.

The dotted areas represent integuments.

(Adapted from Gordon.)

chamber and form what corresponds to the canopy in other later types. The space between the salpynx and the free processes is filled with a loose felt of fine hairs. There is no micropyle. The vascular strand in the pedicel of the ovule is crescent-shaped in section and each process has a vascular bundle which is continuous with the pedicel bundle.

The nucellar epidermis which forms the outside of the pollen chamber consists of thick-walled cells and must have been quite a hard rigid structure. The nucellus is an elaborate structure with its cap of indurated cells prolonged into the salpynx and in this respect more like a Pteridophyte sporangium than the nucellus of any living Gymnosperm.

In the Filicales what has usually been termed heterospory occurs in the Marsiliaceae and Salviniaceae and Arnold has shown that two sizes of spores were formed in at least one species of the Upper Devonian fern Archaeopteris. Recently Surange has proved that *Bensonites fusiformis*, for long considered to be a glandular structure borne on the frond of the Lower Carboniferous fern *Stauropteris burntislandica*, is in part a sporangium which contains two megaspores and that this early fern was heterosporous. *Bensonites* consists of a fusiform body with a terminal sporangial cavity containing the two cutinised spores. The apex of the sporangium is prolonged into a short beak. The cortical parenchyma of the swollen pedicel immediately below the sporangial cavity forms the main bulk of the whole structure. There is no apparent dehiscence mechanism. In some examples it appears as if the beak of the sporangium opened to form a narrow tube apparently too narrow to permit of the passage of the megaspores.

The form of the Salpingostoma ovule with its sheath of vascular processes and its C-shaped pedicel bundle provides considerable support for Halle's theory of the formation of an ovule by investment of a sporangium by adjacent divisions of a frond. In other words an ovule consists of a sporangium or fertile telome invested by a group of adjacent sterile telomes.

This differs somewhat from Dr. Margaret Benson's theory that the ovule was evolved from a bunch of sporangia or a synangium by the outer sporangia or fertile telomes losing their fertility and constituting the integument.

Oliver in 1909 was of the opinion that the integument of the Lagenostomales had been formed by concrescence of a number of originally free structures evolved contemporaneously with the inception of the seed habit and that the integument must be regarded as an organ *sui generis*.

In this early ovule the vascular strand of the pedicel still retains structural evidence of its foliar nature indicating that the sterile telomes had possibly acquired a webbed and dorsiventral relationship similar to a leaf pinnule before becoming involved in the formation of the ovule. It is perhaps of some significance that in the Upper Devonian and Lower Carboniferous when this evolution of ovules was taking place we also find that there is a

distinct trend in frond evolution by a process of webbing from fronds with narrow linear ultimate segments to fronds with broader pinnules. In Lagenostoma the integument is lobed at the micropyle and each lobe of the canopy has its corresponding vascular strand.

The large cupule of Calathospermum contains a considerable number of ovules which differ from Salpingostoma in minor details, nine processes instead of six and in having a cylindrical vascular strand in the pedicel. The cupule consists of six lobes each with several vascular strands. The stalk of the cupule itself has a C-shaped strand which points to the foliar nature of the cupule. It is, however, likely that Calathospermum which has a stalk nearly 4 mm. in diameter may represent a major part of a frond or even a complete frond.

Andrew's reconstruction of a *Sphenopteris affinis* frond bearing numerous Megalotheca or Calathospermum cupules high up on the frond is manifestly wrong because the stalks of the cupules have a much greater diameter than the rachides of the sterile fronds in those positions.

Salpingostoma and Calathospermum are evidently the forerunners of the type of cupule with two ovules as in Gnetopsis and with a single ovule in the majority of cupular ovules. In these early (Lower Carboniferous) types the capture of pollen was effected by the elaborate nucellar apex. The integuments did not form what would be recognised as a micropyle and the ovule does not seem to have been well equipped to remain long in a period of dormancy. The structure of the later types suggest that as the integuments closed in over the nucellus the salpynx became progressively reduced and finally completely disappeared.

Sphaerostoma ovalis, another seed of approximately the same geological antiquity, consists of a cupule containing a single ovule. The cupule and integument have vascular systems. The nucellus bears at its distal end a dome-shaped mass of tissue in which the pollen chamber was developed by shrinkage of tissue below the epidermis round the sides of the dome (fig. 1). This left a central column of parenchyma with the annular pollen chamber surrounding it. There is no salpynx. The free parts of the integument closely arch over the nucellus and form a rather wide micropyle. The coexistence of two such different types of nucellar organisation in the Lower Carboniferous suggests that a considerable range in ovular structure had already been developed.

When we consider the structure of Lagenostoma, the best known of all the Upper Carboniferous types, we see that there is evidence of a short salpynx which projects slightly beyond the very short free apices of the integument (fig. 1).

It would appear that the pollen chamber of Salpingostoma was formed by the disappearance of all the tissue of the nucellar apex with the exception of the superficial layer. In Lagenostoma a central column of this tissue remains. It also suggests that the pollination drop was exuded from the short salpynx and that although a restricted micropyle was formed by the integument the function of pollen capture was still retained by the nucellus. The cupule of Lagenostoma fits very closely round the integument and evidently approximates to an outer integument of the ovule although the seeds were dispersed without it.

Further reduction in the salpynx and modification of the nucellus led to the numerous types of seed included by Seward in the Lagenostomales, for example Gnetopsis, Physostoma, Conostoma (fig. 1) Coronostoma, etc. In these, as far as we can judge without knowledge of the young stages of the ovule, the integument with its narrow micropyle assumed the function of pollen capture.

In the Mesozoic Caytoniales, although as Professor Harris has shown the pollen reached the micropyle, the ovules were almost completely enclosed in the cupule whose entrance was provided with a stigma-like flap and were of relatively simple construction, probably without vascularisation of the integument.

The Trigonocarpales

The seeds included in the Trigonocarpales are large seeds ranging from 1½ to 6½ cm. in length, no cupule has been found connected to any of them.

Hoskins and Cross (1946) have made a detailed survey of the known species of this group including a new species *Pachytesta vera*. The seed in the genus Pachytesta

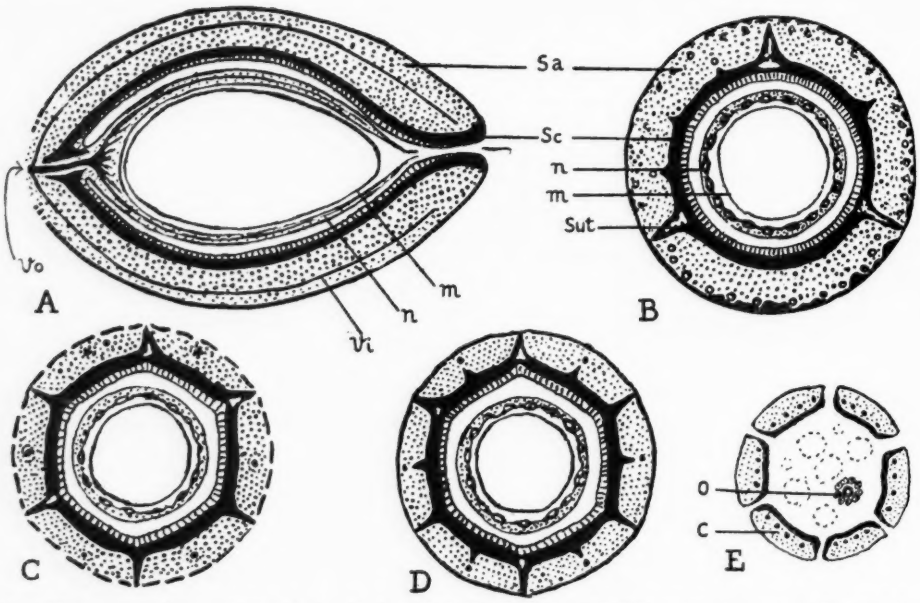


Fig. 2.—Diagrammatic representations of: A and B—Pachytesta, C—Hexapterospermum, D—Polylophospermum, E—cupule and ovules of Calathospermum. A is a median longitudinal section; B–E are transverse.

c, cupule; *m*, spore; *n*, 'nucellus' with vascular tissue; *o*, ovule; *sa*, sarcotesta; *sc*, sclerotesta; *sut*, suture; *vi*, vascular tissue of integument; *vo*, vascular supply of ovule.

(A–D, adapted from Hoskins and Cross.)

(figs. 2A and B) may be as much as 6·5 cm. in length and with a maximum diameter 3·5 cm. The outer covering which is termed the testa has a thick outer sarcotesta, a middle sclerotesta and a relatively thin inner lining of parenchyma. The sclerenchyma of the middle layer projects outwards into the sarcotesta forming three prominent radial ridges on the outer surface of the sclerotesta and there are a variable number of smaller ridges in between the three major ridges.

Thin-walled tissue continuous with that of the inner parenchyma extends into these ridges. It is evident that these ridges represent the sutures of the component parts of the testa. In the closely related forms Hexapterospermum (fig. 2C), Polylophospermum (fig. 2D) and Ptychotesta, there are six main ridges on the sclerotesta. In Polylophospermum there is an additional minor ridge between each pair of main ridges. This somewhat persistent appearance of parts in threes or multiples of three is reminiscent of what is found in Calathospermum, Salpingostoma, Lagenostoma and *Conostoma oblongum*. In Pachytesta as in some Lagenostomales what has been termed the nucellus is free from the testa except at its basal attachment and vascular tissue is present near the surface of the so-called nucellus which extends from the base to the level of the pollen chamber. Similar vascularisation is also present in Stephanospermum and some other seeds which have been attributed to the Trigonocarpales and I shall refer to its possible significance later. In Pachytesta the nucellar apex is prolonged into a tubular beak which projects but a short way into the base of the long micropyle formed by the massive integument and by comparision with the Lagenostomales the nucellar apex and pollen chamber is of simple construction.

The presence of vascular tissue in the 'nucellus' calls for special consideration. It seems to me that there are three interpretations possible. The nucellus has been compared to a sporangium containing one functional spore but we have no knowledge of any other type of sporangium in which vascular tissue is present in the tissues outside the sporogenous tissue. On the con-

trary in Hornea and all other sporangia in which vascular tissue is developed in the sporangial stalk near to the sporangenous tissue it is clear that the sporogenous tissue of the sporangium is developed from what would be vascular tissue in the corresponding sterile part of the plant. If therefore we find vascular tissue in a nucellus it must be of the nature of an *ad hoc* development and of doubtful phylogenetic significance. A second suggestion which, as we are considering fossils, can probably never be put to the test is that in its development the single spore as it enlarged with the development of the female gametophyte within it pushed back into undifferentiated tissues of the stalk behind and occupied a medullary position in the potential vascular tissue. There is a third suggestion which I have to make and which may have greater appeal to some. Is it not possible that the so-called integument of the Trigonocarpales is morphologically equivalent to the cupules of Calathospermum, Lagenostoma and Sphaerostoma and that the outer part of the ' nucellus ' in Trigonocarpales which contains the vascular tissue is an inner integument fused as most integuments are with the nucellus which is very much reduced ?

If we imagine the six cupule segments of Calathospermum (fig. 2E) with their inner region of sclerenchyma fusing together, we would get a sheath in size and structure like the testa of Hexapterospermum and Ptychotesta with their six sutures and six ridges of sclerenchyma. If one ovule instead of the many in Calathospermum was retained as in Lagenostoma and simplified by reduction of its integument and of its nucellar complexity, it could closely resemble the free ' nucellus ' of Pachytesta. The fact that no separate cupule has been found in the Trigonocarpales is understandable if this assumption is made.

The Lagenostomales and the Trigonocarpales together form a group of seeds which have some structural features in common. In the Lagenostomales the nucellus exhibits the highest degree of complexity in its organisation and we have a series of forms with great range in structure. In the more primitive forms the nucellar apex was responsible for the capture of pollen. In what are to be regarded as the more advanced types this function is performed by the integument which has closed over the nucellar apex and left a narrow micropyle.

We have seen that the majority of structurally preserved seeds which are either definitely known to belong to the Pteridosperms or which are related in structure to them fall into the two provisional groups, Lagenostomales and Trigonocarpales. The available evidence suggests that the Trigonocarpales belonged to the foliage genera Neuropteris, Alethopteris and possibly several other somewhat similar form genera which had large fronds. The Lagenostomales were probably borne on smaller fronds, many of the Sphenopteris type, which are found throughout the Carboniferous Succession. The Neuropteris-Alethopteris group of foliage types are more or less confined to the Upper Carboniferous and are probably to be regarded as relatively more highly evolved.

The ovules or seeds attributed to the Pteridosperms can be separated into two groups: the Lagenostomales and Trigonocarpales but they have many features in common and reasons are given for supposing that the Lagenostomales represent relatively earlier stages in ovule evolution than the Trigonocarpales in which simplification in nucellar structure has occurred. There is clear evidence that the integument and cupule in the Pteridosperms have each been derived by concrescence of a number of foliar divisions and it is noticeable in both Langenostomales and Trigonocarpales that the integumentary and cupular parts are predominantly in multiples of three.

There is evident a general tendency for a structure such as the nucellar apex or the integument to be simpler in structure the more closely it is enveloped by some external protective structure. In the Pteridosperms we find that the nucellus has usually a double investment, cupule and integument, both parts with a vascular system. Among living plants many of the Angiosperms exhibit double integuments and several orders have vascular tissue in them in spite of being completely enclosed in an ovary.

The origin of the Angiosperms is still as great a mystery as it was in the time of

Charles Darwin but discoveries made since then give some hint as to the direction in which search may meet with some success.

The vascular Cryptogams and the Cordaitales had by the end of the Palaeozoic become stereotyped and are unlikely to have given rise to anything like an Angiosperm. I think that the same applies to the Bennittitales and Cycadales of the Mesozoic. Dr. Hamshaw Thomas, Professor Harris, the late Professor Sahni and others who have concentrated their attention on Mesozoic floras have given us a glimpse of the wealth of extinct groups of Coniferales, Ginkgoales and many as yet ill-defined groups of Gymnosperms, but again little or no evidence of Angiosperm affinities are evident. We are left with the Pteridosperms, Palaeozoic and Mesozoic and the Gnetales. Hagerup has argued in favour of the latter as possible ancestors of some of the Angiosperms. But it seems to me that in their multiple integumentation and in their leaf-form they have some things in common with the Angiosperms on the one hand and the Pteridosperms on the other. What evidence we possess points to the Pteridosperms having been among all known Palaeozoic forms the most likely progenitors of the Angiosperms.

GREEK OR CHEMISTRY OR BOTH?

Address by

ROBERT BIRLEY, C.M.G.

PRESIDENT OF SECTION L

It is, I think, well known that the best way to sneak into the Annual Meeting of the British Association under false pretences is to become a member of Section L. I certainly feel that I have done so. For I am, most assuredly, no scientist. I was taught some science at school, but I was, I know, the despair of the science masters. For all that they must have taught me well, for I left school with a strange, amateurish interest in science which I have never lost. Above all, the history of science has been for me a particularly exciting subject. I have what is, I realise, a somewhat ridiculous passion for old books, and I am fortunate that there is at Eton a remarkable old library. It is for me a great experience to be able to handle, say, the first quarto of Shakespeare's *Troilus and Cressida*, or the copy of *Dio Cassius* which belonged to Montaigne. But never, I think, have I known anything so exciting as when I pulled a book out of one of the shelves and found that I held in my hands a copy of the original edition of the *De Revolutionibus* of Copernicus. The late Sir Henry Marten, first a master at Eton and later Provost of the College, who taught me all I know about teaching history, once entered the great Cathedral of St. Vitus in Prague and, as he did so, startled the worshippers and the sightseers by exclaiming in ringing tones, 'This *is* history.' That is what I felt at that moment. Here in my hands was history, a book which had changed man's outlook on the Universe and on his own position in it.

That outlook was not changed all at once, of course. I suppose it is true to say that, until Galileo had destroyed Aristotelian physics, the opponents of Copernicus often had the better of the argument. But what seems to me so striking, when reading of the great scientists of the Renaissance, was the closeness of their relationship with the whole of the educated world of their day. Any educated man understood what the problem was. Copernicus, using the term metaphorically, spoke the same language as, let us say, Sir Philip Sidney. Their early education had been very similar; their cultural background was the same. I always enjoy the story that tells how Copernicus was first led to think of his theory that the earth moved round the sun by hearing, when a student at Cracow University, at a lecture on Latin literature, the line of Virgil,

> Provehimur portu, terraeque urbesque recedunt.

(We set out from port and the lands and towns move from us.) The story may be apocryphal, but at least it is quite possible. Or one may think of the charming game which Galileo played with his scientific friends, when on December 11, 1610, he sent to Guiliano de Medici the following cryptic sentence, 'Haec immatura a me jam frustra leguntur,' adding the letters O.Y. (These immature matters are read by me in vain.) It was known that Galileo delighted in anagrams, and the unfortunate Kepler wrestled long with this one, and eventually produced the startling statement, 'Macula rufa in Iove est, gyratur mathem." (There is a red spot on Jupiter, it rotates mathematically.) In the end Galileo disclosed the answer, 'Cynthiae figuras aemulatur mater amorum.' (The mother of loves—*i.e.* Venus—emulates the figures of Cynthia—*i.e.* the moon.) He had discovered that the planet, Venus, had phases like the moon. The fact that he was quite wrong in believing that this proved the truth of the Copernican

system is immaterial to my purpose. What is interesting is not only that Galileo used naturally language which would have been familiar to the classical scholar. For who can doubt that, when Copernicus, to complete the story, heard that line from the *Aeneid* and was by it reminded of how he had once set sail down the Vistula and seen the shore move from him, or when Galileo spoke of the planet as ' the mother of loves ' and of the moon as Cynthia, they felt themselves to be akin to the civilisation which had produced these beautiful images ? In the deepest sense of the phrase, they were using a living language.

We can understand the mathematics used by Copernicus, and appreciate the patient zeal for experiment shown by Galileo. But what seems to us now so strange is the unity of culture which in those days bound together the classical scholar and the scientist. Galileo studying the planet through his telescope is a phenomenon with which we feel quite familiar, but Galileo using classical allusions in a scientific joke seems beyond words bizarre. For the humanist and the scientist of our time very rarely, if ever, use the same language. In the last three hundred years, a great rift has appeared in our culture, and it is of the rift and its educational implications that I wish to speak to-day.

Let me admit two points at once. No one could suggest that the modern scientist will gain anything by adorning his theses with classical allusions. It is not only in laboratories, but in other places, such as the House of Commons, that classical quotations have gone out of fashion. Further, modern science is now so technical that no one could hope that the ordinary well-educated man could understand a scientific treatise to-day, as Sir Philip Sidney could understand Copernicus. How different it is now from the science of Dr. Johnson's time, for instance. Dr. Johnson found chemistry ' enchanting.' Boswell, going into the great man's library for the first time, ' found a number of good books, but very dusty, and in great confusion. The floor was strewed with manuscript leaves. . . . I observed an apparatus for chymical experiments, of which Johnson was all his life very fond.' But, then, in those days, ' chymical experiments ' were simpler than they are now. Boswell tells us how he once visited Johnson who was engaged in dictating to Mr. Peyton, his amanuensis. ' He seemed also,' Boswell continued, ' to be intent on some sort of chymical operation. I was entertained by observing how he contrived to send Mr. Peyton on an errand, without seeming to degrade him. " Mr. Peyton—Mr. Peyton, will you be so good as to take a walk to Temple Bar ? You will see there a chymist's shop; at which you will be pleased to buy for me an ounce of oil of vitriol ; not spirit of vitriol, but oil of vitriol. It will cost three half-pence." ' The scientist to-day deals in more complicated materials, and he needs all the help he can get from the University Grants Committee to purchase them.

I am not going to suggest—to use language with which we are familiar—that the classical specialist should continue the study of chemistry until he takes it at advanced level in the examination for a General Certificate of Education, or, for we shall have to consider also the other side of the medal, that the scientist should study Greek to the same standard. For all that, I believe that we must face the existence of this dichotomy in our culture, even though we realise that we cannot possibly close it as completely as in Dr. Johnson's time, or in Galileo's. We might express it in this way. There are to-day in our society two classes who have perhaps the greatest influence. On the one side there are the executive civil servants. Many of them will have received a classical education, nearly all an education based on the humanities. On the other side there are the scientists. Their educational background will have been quite different. From the age of fifteen or sixteen, in this country, they will have concentrated on scientific studies. Probably for some time before they will have studied more science than the boy or girl who has another specialist subject in view. Both will have undergone a severe, in fact a splendid, intellectual discipline. But the education of the civil servant will have been intended, fundamentally, to enable him to understand Men, while that of the scientist will have been intended to enable him to understand Nature. It is hardly necessary to exaggerate the seriousness of

the effects of this difference, but perhaps it is worth saying this. The civil servant will have to deal with a world largely dominated by the effects of scientific discovery : his education will have done nothing to help him to understand—not the technique of the scientist, for that we must agree is not to be expected—but his attitude and his approach to his subject. The scientist will find himself faced by immense social responsibilities as a result of the power which science provides : his education will have done nothing to prepare him to shoulder these responsibilities.

I have kept the question so far on a strictly practical level. There are, however, more profound implications in this dichotomy. I believe that the sense of helplessness which is the most disquieting symptom in our civilisation is largely derived from it. On the one side men who have social responsibilities feel helpless before a power which they cannot control or understand : on the other side men who control the power feel that they cannot concern themselves with the social responsibilities in which it involves them.

The existence of this rift in our civilisation is, of course, widely recognised, and it has been recognised by many that it sets an educational problem. This is tackled, if at all, at the Universities. The Universities seem to be much more fully aware of it in the United States, France and Germany than we are in this country. The General Studies in American Universities, the 'année propadeutique' in France, the 'Studium Generale' in Germany are all constructive attempts to find a solution. But they do not seem to me to get to the heart of it. Lectures on general subjects, which means, of course, those subjects outside one's specialist study, can hardly appear of serious importance to a University student intent on his main work. Above all, they nearly always entail the continued segregation of those on the two sides of the great divide.

When there are rifts in a culture there is always a danger that it will be thought that they are closed when all that has been done is to paper over the cracks. It seems to me that lectures on general subjects at a University are a perfect example of 'papering over the cracks.' The rifts are not really closed. The only way in which they can be closed is, I believe, by those on either side actually working together. And this, it seems to me, cannot be expected at the Universities. The problem can only be solved (in so far as it can be solved at all : it is so immense that only a partial solution is possible in our own times)—it can only be solved in the Secondary Schools.

I feel that, at this point, you may begin to expect that I am going to suggest the disappearance of the specialist work in the Sixth Form which is the most distinctive feature of the English Grammar School. I am not intending to do so for two reasons. First, it seems quite obvious that the development of scientific technique has made it quite impossible to expect the Universities to spend their time on the elementary teaching of science. Secondly, I believe that the specialist work in a Sixth Form is educationally of great value. It seems to me that an intelligent boy or girl, after the age of about sixteen, needs such work. He has reached the stage when he can find how exciting it is to learn to master the subject of his choice. To keep him from it would seem to me to be simply to frustrate him.

I do feel, however, that there is much truth in the charge often levelled against the Grammar Schools that they allow, or even encourage, boys and girls to start specialist work too soon. As will be seen, the suggestions I shall make will involve a rather later date for the beginning of specialist work than is now often common. And, perhaps I may say now, as the dust of controversy has pretty well settled, that I believe that the fixing of the minimum age of sixteen for the General Certificate was really a wise move. It was vitiated, of course, by the failure of the Universities to respond to it by making the necessary consequential changes in their entrance requirements. As high a standard in the specialist subjects was demanded as before. The unfortunate teacher found himself in the position of a figure in some horror story with the walls of the room in which he was imprisoned closing in on him from both sides. But, if I may be personal for a moment, I am glad that I am headmaster of a school where traditionally the School Certificate was taken late. When

the minimum age limit was imposed, I found that my friends who were headmasters were faced with appalling problems in rearranging the curriculum. As far as I remember it cost me one evening's work. Which only shows, perhaps you may say, that, if one remains quite stationary for long enough, one finds oneself eventually in the van of progress.

May I crave your forgiveness and refer to another personal experience? I am approaching practical matters, and it is the best way that I can do so. Before the war, I was headmaster of Charterhouse, and we had there an arrangement by which most of the ablest boys worked together until they took the School Certificate, for which we had our own private minimum age of fifteen and a half. They all did the same work : all learnt Greek and all learnt Chemistry. In 1938 we were inspected by the Board of Education. I displayed to them this form, the apple of my eye, but it was rather dubiously surveyed. ' Your future scientists,' I was told, ' surely they ought to start at least semi-specialising in science rather earlier. They ought to do rather more science than the others at fourteen and a half.' And then came the 'cri de cœur,' and here I am quoting the exact words used, ' You are missing your border-line Science Exhibitions.' (May I hasten to add that I am quite certain that no Inspector from the Ministry would say that now.) For the first time in my life I realised fully the importance of the economic factor in education. I was able to snap my fingers at them, and I did so, because the endowment of the founder of the school made it possible for me, in practice, to help all those ' border-line ' failures to go to the University. But supposing I had been in the shoes of my friend, the headmaster of Godalming County School across the valley. I should have had to surrender. For it was perfectly true : we were missing our border-line Science Exhibitions. You will see, in due course, that I appreciate that the suggestions I have to make will involve some relaxation of the entrance requirements of the Universities, though not, I believe, a very great one.

' Greek or chemistry or both ?'—I used the words advisedly. I am going to consider only the ablest children, partly because I have no time to do more, but chiefly because I believe that for this purpose, though not in the eyes of God, they are the most important. They will create the culture of the future. I take Greek because it is an extreme case. It is the most exacting humanist study : in most schools it is only studied by really able boys and girls. I am not, in fact, imagining that they will all learn it, but it will serve my purpose as a term of art. Chemistry is not in quite the same position : any boy or girl who takes a course in general science will learn some chemistry. But my friends who are science teachers tell me that it is the branch of science which creates most difficulty in the early stages and that it is the one which involves the longest and most intensive study before specialist work begins.

The rift, I said, can never be closed by papering over the crack. Those on both sides must have worked together, must have had a common intellectual experience. This is impossible now at the University, and, I think, it is difficult to do much about it in the Sixth Form. But I believe that something would be done to close the rift if in the Grammar Schools it was established that the ablest boys worked together for three, four or five years before Sixth Form work began. I realise that one could not go the whole hog. Doubtless in mathematics they could hardly be expected to keep together. I should, I confess, like them all to learn Greek, but I am aware that to ask for that would be to cry for the moon. But I should insist on their all doing science together, for I believe that the inability of the civil servant to understand the scientist is a more serious danger even than that of the scientist to understand the civil servant.

Something would be lost, of course. The disappearance of ' semi-specialising ' would inevitably lower the standard of specialist studies in the Sixth Form, though not as drastically, I believe, as might be thought ; this would presumably entail some lowering of the entrance standards at the Universities. I do not really believe, however, that it would in the end mean that we should have less able or well-qualified civil servants and scientists.

But I think we should go rather further.

It would seem to me most unfortunate that the scientist should do no more Greek, the classical scholar no more chemistry after this period of common study was completed. I speak now with some hesitation, as I have little in the way of personal experience to go on, but what I have had leads me to suppose that an arrangement on the lines I shall suggest would be feasible. I should like to see the ablest boys—and, for that matter, all in the Sixth Form—again working together for a few periods a week. During these periods they would for a term sometimes study humanist subjects, sometimes scientific, all of them, classical and science specialists alike and together. The technique of instruction would have to be very different from that of most Sixth Form work. It would be almost true that the best advice to the teacher would be, ' Do not mind being superficial,' though, as all the children would have learned the grammar of the subject, it should not be impossible sometimes to reach deeper levels. Whatever it was the work should never be, to use the old nomenclature, ' rather more advanced School Certificate work.' The study of the Classics, for instance, should aim at exploring the minds of the Greeks or Romans—but it could be done in the original texts ; the study of science should aim at showing how the scientist thinks and how he approaches his subject—but the pupils would have some understanding already of the terms and methods employed.

The boy or girl specialising in science, it may be said, would waste his time during the term when a scientific subject was being taught. I believe that he might in this way gain insights into his subject which he would never find in his ordinary specialist work. But, in any case, he would do valuable work in teaching his humanist fellows. In due course he would have his reward. For next term, when the form was reading quickly through six books of the Iliad, it would be the classical specialists who would be expected to construe all the difficult passages.

Perhaps I might be allowed again, and for the last time, one personal reminiscence. I have attempted to teach the history of science to a form containing boys specialising in mathematics, science, classics, history and modern languages. For myself, you will understand, the presence of the science specialists was an absolute necessity. It gave me a peculiar pleasure to see the look of pained attention on the faces of those boys who specialised in classical or modern languages or history, as a mathematical specialist gave a most lucid explanation of the rainbow during a lesson on Grosseteste, or a biology specialist discoursed on the circulation of the blood during one on Harvey.

And now, finally, I must consider two objections, both formidable, the latter perhaps almost insurmountable. How, it may be asked first, will all this fit into the requirements of the General Certificate of Education at Advanced Level, as required for State Scholarships and County Awards? And the answer, of course, is that it will not do so. How then is one to make a start ? We face here one of the most serious problems in our present day educational arrangements. Forget, if you like, for the moment the suggestions which I have made. Put in their place others, and better ones, of your own. The difficulty would be the same. That patient, hardworking and self-effacing body of men and women, the Secondary Schools Examination Council, must fit the examinations to the requirements of the school. We all demand it of them. But what is to happen, then, if some individual school has different requirements ? And where shall we be in the end if no school ever changes its requirements ? For all the great educational movements of the past have taken place in response to the changing needs of society, and nothing, not even the Secondary Schools Examination Council, will stop society from changing. This difficulty seems to me so serious that it leads to the inescapable conclusion that our whole magnificently efficient system of external examinations is really the greatest danger that exists to English education. Sooner or later, I feel sure it will have to be done away with.

The second obstacle is even more formidable. It seems obvious that any changes on the lines I have suggested will make necessary more teaching of science in the Grammar Schools, and this will mean more teachers of science. And

everyone knows that there are not nearly enough as it is. Or do they know it? Nothing is more baffling to the Grammar School master or mistress, especially the headmaster or headmistress who has to engage the staff, than the apparently total inability of the country as a whole to appreciate the fact that there is a disastrous shortage of able teachers of science. I do not want to trouble you with statistics. There is, in any case, no time for them, and to be of any value they require careful analysis. I shall refer only to these, which I take from a most cogent article, printed in *Nature* last December. In July, 1952, replies to an inquiry from three-quarters of the Grammar Schools, including the Public Schools, in England and Wales, showed that there were 49 Science posts unfilled at Boys' and Mixed Schools, and 116 in Girls' Schools: 116 vacancies is about one-fifth of the total number of science posts in the girls' schools. In 1950, out of a possible 213, only 11 science graduates at Oxford elected to take up teaching, and of these 11 none had a first-class honours degree, and only three second classes. Before the war, any of the great Grammar Schools of the country would expect to have three or four men with first-class honours on its science staff.

If anything that I say to-day is reported in the press, I hope it may be this, that the shortage of science teachers in the schools is so serious that it is quite futile to talk of the development of the scientific strength of this country, or to plan Institutes of Technology, or anything else of the kind, until it is remedied. I said before that we could not expect the Universities to spend their time teaching elementary science. They will be doing so before long.

The reasons for this desperate state of affairs are no doubt largely economic, and it would be blindly foolish of the nation not to ensure that the salaries of masters in the Grammar Schools are raised to a figure high enough to attract able scientists into the teaching profession. But there are, I believe, other causes also at work. I sometimes suspect that the inability of the scientific profession to produce enough teachers is evidence of some underlying failure of faith among scientists in science itself. For it is a sign of faith in a subject to wish to teach it to others. The education we provide for future scientists, however, seems to me to be another of the causes with which we must reckon. It has been too technical and too narrow. For teaching itself is not a science, but an art and it needs, for those who engage in it, a humanist education. In this great divide in our culture, the teacher is inevitably on the humanist side. Thus the rift, of which I have spoken, created at least in part by the kind of education our schools provide, is beginning to affect the schools themselves. This is but one sign of it, but it challenges us here, who are concerned with education, with what is, in my view, the most serious educational problem of all those which we have to face.

APPLICATIONS OF "ANIMAL PHYSIOLOGY" TO AGRICULTURE

Address by

DR. J. HAMMOND, C.B.E., F.R.S.

PRESIDENT OF SECTION M

WHILE pure science goes very deeply into a very limited problem—so that the definition of a scientist has become one who knows more and more about less and less, the function of an applied science is to synthesise this detailed knowledge into a constructive whole so that it can be used for a specific purpose. Thus, animal physiology as a basal science for animal production has to co-ordinate the results of pure scientific research in anatomy, zoology, genetics, physiology, endocrinology and biochemistry and determine the interactions between these so that control over animal production can be obtained. In doing this it is often found that there are large gaps in knowledge between the pure sciences, for example between anatomy and physiology in the methods of control of the conformation of animals, which need to be filled in by research by the applied scientist or put before the pure scientist in the form of a problem which he can appreciate. Examples of the necessity for co-ordination of the findings of the different branches of biological science will be given later.

While present-day medical physiology supplies animal physiology for agricultural purposes with some useful techniques, it has become far too narrow in its outlook and deals too much with critical short time happenings in the animal body and too little with the chronic lifetime results which are so important for animal production. By going back to the earlier works (about 1880), such as those of Luciani on human, Colin on comparative, and Meade Smith on veterinary physiology one obtains a much wider outlook more applicable to animal production than the narrow fields covered by medical and veterinary physiology to-day.

In animal physiology, as in fact in most of our agricultural sciences in this country, the beginnings go back to the work of Lawes and Gilbert at Rothamsted. They studied the economics of animal production on the farm, in this case that of fattening animals, and then investigated the problem by scientific experiment. This line of approach could well be adopted by more of our young agricultural scientists to-day who tend to work in the laboratory only. While until recently no attempt has been made to set up a research institute specifically for the purpose of the study of animal physiology much work on this subject has developed in research institutes for animal nutrition, dairying, genetics, and grassland, for it has been found that a knowledge of the workings of the animal body and its reaction to environmental conditions is essential for an advancement of their subjects.

While in the faculties of agriculture at our universities it has long been recognised that physics and chemistry form the basal sciences on which teaching in soils and manures is founded, and botany the science which is basal to crop husbandry, the recognition that physiology (including nutrition, zoology and genetics) is the science basal to animal husbandry has been very limited and until recently teaching in this subject has been founded for the most part on traditional farm practice. There is need for this outlook to be changed.

While the objects of medical and veterinary physiology are the study of the normal workings of the animal body to enable one to detect abnormalities and the effects of

disease, animal physiology has as its objective the obtaining of control over the functions of the animal body in order to increase the output of eggs, offspring, milk, meat, and wool, and to maintain good health throughout a long life of high production. It is therefore supplementary to medical and veterinary physiology although it interlocks with the latter on questions of sterility and nutritional disorders.

Now let us consider some of the ways in which animal physiology, a study of the workings of the animal body, can be applied to the problems of animal production. In so doing reference will be made preferentially to those problems in which active research is now in progress.

The rate and efficiency of the process of reproduction is the first basis of economic animal production as Heape in his book on *The Breeding Industry* (1906) pointed out. Although the economic outlook has changed somewhat since his time and the low fertility rate in horses (60 per cent.) is no longer so serious as it was in his day, the fact remains that some 20 per cent. of all our losses from dairy herds are due to sterility and at the best the conception rate is only 70 per cent. in cattle, which means loss of time and milk supplies. Moreover, the lambing percentage, which in many areas in Australia is often as low as 70 per cent. and under our intensive conditions in this country is in many cases below 150 per cent., forms a major check to increased output and economic production. The knowledge on gonadotrophic hormones which has accumulated in recent years has given us an insight into the causes of, and a method of control over, many of these problems of fertility. By intravenous injections of luteinising hormone we can control the time of ovulation and so increase fertility in horses (Day) and by subcutaneous injections of follicle stimulating hormone some four days before the onset of heat increase the number of eggs shed in sheep ; some promising preliminary experiments in this country and in New Zealand have indicated that the lambing percentage can be increased by this means.

Experiments with these hormones in causing many eggs to ripen instead of one or two have shed light on old problems and have opened the way to completely new developments in animal husbandry. While in the cow as many as twenty-six fertilised eggs have been obtained at one heat period, it is found that not more than three or four calves can ever be obtained at a birth because fertilised eggs in excess of this number die. This early death of fertilised eggs is a major problem in normal matings in pigs and cattle and is probably why in cattle even under optimum conditions a conception rate of about 70 per cent. is rarely exceeded (Laing).

Although the ovary of the new-born calf contains on the average some 70,000 eggs, rarely do even the best of our cows produce more than ten calves in their lifetime. By superovulation and transplantation of the excess eggs into other females to incubate it should therefore be possible to increase the reproduction rate of genetically superior cows as artificial insemination has done for bulls. Moreover, it should be possible to make our dairy cows, from which replacement heifers are not required, produce pure bred beef calves to support our beef industry. Pilot experiments with rabbits have shown that about 80 per cent. of transplanted eggs produce normal young at birth, but the technique requires a major operation. Using a similar technique in cattle Willard and others in U.S.A. have obtained three calves from five eggs transplanted. So far attempts to transplant by an inseminating tube into the uterus four days after heat have failed owing to the susceptibility of the uterus to infection when under the influence of progesterone (Rowson, Lamming and Fry) and this problem has yet to be solved before transplantation becomes a practical proposition in cattle. When it does become a practical proposition, however, the scope for speeding up genetic improvement in our farm animals will be much increased, for it has been found that, by the use of gonadotrophic hormones, fertilised eggs can be obtained from the newly born calf so that a generation of cattle per year is a possibility to consider (Marden). With rabbits it has been found possible (Chang and Marden) to keep the fertilised eggs alive for four days outside the body and to transport them successfully across the Atlantic by air.

As soon as the problem of the technique of transplantation in the cow has been overcome it will be possible with the modern jet plane to export anywhere in the world whole herds of pedigree cattle as fertilised eggs for as many shillings as it now costs pounds for a single animal.

In some species and breeds of animals (such as the mare in summer, and ewe in winter) which have a breeding season regulated by the periodic output of gonadotrophic hormones from the anterior pituitary, control can be obtained over them through changes in the darkness-daylight ratio. This acts like slow-motion Morse code signals to nerves in the region of the head, stimulating or depressing the action of the anterior pituitary gland and so the output of gonadotrophic hormones. By giving electric light after dark during the winter months mares can be made to breed earlier in the spring (Burkhardt) and by giving more darkness during the summer months ewes can be made to produce lambs in the late summer and autumn (Yeates). The effect of lighting fowl houses on the winter production of eggs is well known, and depends on the same mechanism. One of the limitations of sheep in intensive animal production, where the demand is for lamb carcasses, is its low rate of reproduction per year as compared with pigs, and the solution of the problem, not far distant, depends on methods of controlling the gonadotrophic hormones. In the long run, it will probably be found more economic to do this by breeding than by light treatment or injections, but the knowledge of the mechanism which controls it will enable selection for this character to be made more efficiently.

Physiological studies of the male reproductive processes which have resulted in the technique of artificial insemination are well known, and its application to the mass improvement of our livestock has already been considerable. It is now possible for the small dairy farmer to keep another cow in place of a troublesome and unproductive bull and to obtain insemination from a progeny tested bull which will ensure that his heifer replacements are genetically superior to their dams. Moreover, if he inseminates the worst third of his cows from a ' colour-marked ' beef bull he can ensure that the bad characters in his dairy stock will be eliminated without cost and at the same time produce calves to form an economical basis for beef production. Artificial insemination has put a most potent weapon into the hands of the animal breeder.

As foreshadowed by Parkes and Polge at the Edinburgh Meeting of the British Association in 1951 deep-freeze semen (– 79° C.) by the glycerine method has now become a practical possibility and many calves have already been born as a result of its use. So far as the experiments have yet gone (Polge and Rowson) the deep-freeze semen remains as fertile as it was at the time of collection up to a year, and how much longer we cannot yet say. Deep-freeze semen can be diluted up to 1 : 100 without loss of fertility. This will do many things : it will enable a nominated insemination from any bull in the country, or for that matter throughout the world, so that the man with a small herd can become a pedigree breeder using the best bulls in the world wherever they may be situated, for little extra cost. It will also enable the very best progeny tested bulls to be used much more extensively for every drop of semen can be used, so that if thought desirable all the cows in this country could be inseminated from a hundred or so bulls. Moreover, it will extend the use of artificial insemination in those countries such as New Zealand and Ireland where calving is concentrated in two spring months, for the semen can be collected regularly throughout the twelve months and used during the two months when it is required. If it should be found possible, as it would appear theoretically, to store the semen over a number of years the effect on genetic improvement would be tremendous. Moreover, it will enable semen, as well as animals, to be put in quarantine and so facilitate distribution throughout the world.

Turning now to the question of milk production, the study of animal physiology and particularly of hormone action, has much to offer. The neuro-hormone-muscular action of the ' let-down ' of milk investigated by Petersen and others has led to the development of better milking techniques and machines. Studies of milk pressures in the udder which slow

down the rate of secretion and eventually lead to drying off have shown the necessity for either milking more frequently or breeding for more elasticity in udder tissues if higher milk yields still are required on twice-a-day milking. Perhaps, however, the greatest advances yet to be made are in the application of the hormones to the growth of udder tissue. A beginning has been made in that virgin heifers can be brought into milk in the course of six weeks or so, by the implantation of tablets of the synthetic oestrogens such as stilboestrol (Folley and Malpress, Hammond Jnr. and Day) and yields of up to 1,000 gallons per lactation have been obtained, although at present there is great variation in the results. Freemartins and steers can also be made to give milk by this method, but the amount is small owing to the small duct development in early life. When hormone control over this early duct growth stage of development is obtained it should be possible not only to make bad milking heifers into good ones, but also if required to use steers for milk production.

An example of the way in which a study of animal physiology can co-ordinate the results of two different sciences, endocrinology and nutrition, is seen in the development of the udder during the later stages of pregnancy. Endocrinology has shown that it is the oestrogens circulating in the blood which stimulate the mammary tissue to growth during the latter part of pregnancy, but Wallace's experiments with sheep have shown that the quantitative results obtained by this hormone stimulus depend not only on the amount of the hormone present, but also on the plane of nutrition of the animal at the time the stimulus is applied. Thus ewes which gained 40 lb. in weight over the last two months of pregnancy produced 5 gallons of milk a week after lambing as compared with only 3 gallons from those which lost 10 lb. body weight during this period. The same thing was found to apply to the results obtained by a given injection of gonadotrophic hormone on the ripening of follicles by calves (Marden) or young rabbits (Adams). The well-known effects of flushing ewes before tupping in increasing the lambing percentage is another example of the same thing. In all these cases high plane nutrition is quite ineffective unless combined with hormone action.

Another field in which the study of animal physiology offers great possibilities is that of growth and meat production. The development of early maturity in our livestock, which from a farming point of view is very desirable, as it means a quicker turnover on capital, throws the emphasis more and more on the maternal characteristics of our breeding stock. For example, the fat lamb slaughtered at 16 weeks old is dependent on its mother for nutrition not only completely during the 5 months of pregnancy and first 3 weeks of post-natal life, but partially up to the very time of slaughter. The pork pig at 120 lb., killed at about 4 months old, is also dependent on the sow for 4 months of pregnancy and 2 months of its post-natal life.

Since the efficiency of conversion of feeding stuffs is much greater in the young animal than in the older one, it is important that we should obtain a measure of control over the animal at this time. As has been shown by Wallace, Thomson and others, the control of the plane of nutrition of the ewe during the later stages of pregnancy is an effective way of controlling the size and development of the lamb at birth, as well as of affecting its milk supply after birth. If the ewe is well fed during the last 2 months of pregnancy she will at the 105th day after conception produce twin lambs of 12 lb. each which anatomically and physiologically are 165 days old, while if she is poorly fed during this time she will produce weakly lambs of only $7\frac{1}{2}$ lb. which anatomically and physiologically are only 130 days old. Such lambs are ' premature ' in development and as a result of this their temperature regulation mechanism is not yet perfected, and many die of cold in this country. Thousands of lambs born in flocks of hill sheep perish each year from this cause.

Another method of control is genetic. There is a maternal influence over the size of the young at birth as shown by experiments on reciprocal crosses between the large Shire horse and the small Shetland pony. Our dairy breeds of cattle such as the Friesian have large calves at birth and a quick rate of liveweight

increase, whereas in our beef breeds such as the Hereford and Aberdeen Angus the calves and subsequent rate of liveweight increase are smaller. By mating Hereford bulls or Aberdeen Angus bulls with Friesian cows we can obtain the best of both worlds, by combining the easy fattening qualities of the former with the size and quick growth of the latter.

In the matter of efficiency of conversion of feeding stuffs the new-born calf because of its high initial growth capacity is, like the newly calved cow, a most effective converter of feeding stuffs into human food, and as experiments in progress by Brookes have shown it pays well to put the young calf on to a high plane of nutrition, for it will shorten by one year the return on capital invested in the beef animal.

The meat qualities of an animal depend among other things on the proportion of the carcass to liveweight, and on the proportion of muscle, fat and bone in the carcass. As the animal grows up there is an orderly change in these proportions, the cranium, shanks, and bones being early maturing, while the loin, muscle and fat are late maturing. A measure of control over the proportions of the body can be obtained as shown by McMeekan for pigs, and Verges and Pálsson for lambs, by changing the plane of nutrition on which the animal is reared at different phases of its growth. For example, as the rate of length growth of a bone occurs early in life and thickness growth later, rearing on a high-low plane of nutrition gives long thin bones, while rearing on a low-high plane gives short thick bones. This work on the plane of nutrition in relation to growth has shown that a generalisation can be made. The incoming nutrients in the blood stream are partitioned to the different tissues or parts of the body in relation to their rate of metabolism at the time. Thus on low planes of nutrition in early life the early developing parts such as bone length and head have first call on nutrition and so the growth of the later developing parts such as bone thickness and loin are inhibited, thus giving an animal which lacks the characteristics of blocky form and high proportion of loin which are characteristics of the good meat animal.

These findings of physiology on the partition of nutrients between the different tissues and parts of the body have a most important bearing on the practice of genetic selection for meat and milk qualities, for the later developing the part or tissues of the body concerned, the greater is the influence which can be exerted by the plane of nutrition. That is, the coefficient of heritability of a character is greater in early maturing parts and tissues such as head and bone and least in late maturing parts such as udder development and fat. The full genetic expression of these later maturing characters of the body cannot be obtained unless the plane of nutrition no longer becomes the limiting factor. Let us take an example to illustrate this : the colour of butter fat is controlled by genetic factors, but since the amount of colouring matter in the butter fat is also controlled by the green colouring matter in the feed, if this latter is omitted no genetic difference can be observed at all. As we increase the amount of green colouring matter in the feed the colour of the butter fat rises until the limit of intensity of colour, the ceiling value, set by genetics is reached. We can then make accurate genetic selection for any shade of butter colour which we desire. It is just the same with milk yield or body conformation and composition. If we fail to ' steam up ' our heifers properly and give them insufficient food in the later stages of pregnancy how can we hope to obtain a measure of their genetic capabilities for growing an udder and producing milk ?

Animal physiology provides an essential basis for livestock improvement and genetic selection in that it shows what the factors limiting production are and how they may be overcome. For example, in many of our well-bred and well-fed cows to-day the limit of production is imposed by milk pressure in the udder, which can be overcome either by more frequent milking or by the selection of cows with more elastic udders. Again, although we have made much genetic improvement for milk production in this country, when our dairy cattle are taken to the Tropics we find that they give far less milk even though they are adequately fed. This is because their body temperature regulating

powers are inefficient in air temperatures above about 75° F. Breeds natural to the Tropics have by natural selection acquired a tolerance to high air temperatures and this character has to be added to our breeds before they become efficient under tropical conditions. Work is in progress to determine exactly how the power of heat tolerance is obtained in tropical cattle.

The physiology of body temperature regulation has important agricultural applications in our climate also. In all species the young animal has less cold tolerance than the adult, due in part to lack of subcutaneous fat. Reference to this in lambs has already been made, but it is in the pig, with its lack of hair covering and earlier stage of development at which birth occurs, that the greatest losses occur. These have been estimated by some to be as high as 25 per cent. shortly after birth. Moreover, unless kept free from draughts and at air temperatures of 75° F. for young pigs and 65° F. for older ones growth is slowed down and the feed consumption per lb. of liveweight gain is increased. It has been estimated that under our winter temperatures it requires about 1 cwt. more meal to produce a bacon pig than when temperature conditions are optimal.

The effects of various environmental conditions on the productivity of the animal and the means by which the animal adjusts itself to its environment offer much scope for profitable study. For example, which type of fleece is best for high rainfall areas and whether in such areas a small animal is desirable due to the general lack of calcium and phosphorus in such areas. The effects of daylight-darkness hours have already been mentioned in connection with the breeding season in sheep and horses and it has recently been found that this rather than temperature affects the rate of growth of wool and is the primary cause of the hair moult.

Much work has already been done on the physiology of nutrition but there is still much more to unravel. Of particular interest at the present time is the way in which vegetable proteins are converted to animal proteins in view of the shortage of the latter available as feeding stuffs. In this process the importance of vitamin B_{12} is evident and while this is synthesised by organisms in the rumen of cattle and sheep and so has a chance of becoming absorbed, in pigs and poultry cellulose digestion occurs in the caecum so there is very little chance for absorption and it has to be supplied in the feed. The fact that cobalt is an essential element in vitamin B_{12} makes ruminants particularly susceptible to its deficiency in feeding stuffs. Recent work on antibiotics seems to indicate that they can act as a supplement and substitute for vitamin B_{12} and so will form a welcome addition to our supplies of B_{12} and animal proteins. It may be found that the growth hormone of the anterior pituitary which encourages the retention of proteins by the body is associated with this story.

A recent addition to the realms of animal physiology is that of the study of animal behaviour which promises to create a science of the ' stockman's art.' Studies on the grazing habits of cattle and sheep show promise of finding ways and means of increasing production by ensuring that the grass is at the right height for maximum intake for each species and possibly by feeding after rather than before milking. The effect of ' conditioning ' the animal for the proper ' let down ' of milk has already been referred to, while methods of the overcoming of ' inhibitions ' built up in bulls against natural or artificial service have already proved most useful.

This brief outline of some of the ways in which the study of animal physiology in its widest sense can be applied to the problems of animal production is sufficient to show the need for the increased extension of such studies of methods of control over animal functions if we in this country are to develop an intensive animal production —as indeed we must if our people are to enjoy the good things in life, for with a dense population and little land at our disposal we must either intensify production, or be content with a much lower standard of living. Scientific applications to animal production are for us, more than for any other nation in the world, a sheer necessity.

Printed in Great Britain
SPOTTISWOODE, BALLANTYNE & CO. LTD.
London & Colchester